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MYSTERIES OF TIME AND SPACE

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RICHARD A. PROCTOR

AUTHOR OF 'PLEASANT WAYS IN SCIENCE' 'THE EXPANSE OF HEAVEN'
'FLOWERS OF THE SKY' ETC.

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> 'One music as before, but vaster'
> TENNYSON

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PREFACE.

THE ESSAYS in the present volume are for the most part among those in which I have endeavoured to present the tendency of science in our time to show the domain of law as to all intents and purposes infinite. The mysteries of time and space here dealt with were no mysteries to those men of old times who supposed that at a short distance from this earth lies the region which they regarded as the heaven of heavens, and that a short distance in time from the present takes us to the beginning of all things in the past, to the end of all things in the future. Starting with such conceptions—the positivism of early thought—men found more and more of mystery as they learned more and more of actual facts, until *now* that which of old was confidently explained is found to be utterly inexplicable, save as a part—an infinitely minute part—of the mysterious Infinity and Eternity we call The Universe.

RICHARD A. PROCTOR.

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MYSTERIES OF TIME AND SPACE.

NEWTON AND DARWIN.

It is singular that the theory which—of all those advanced since Newton established the law of gravitation—has given to thoughtful minds the grandest conceptions of Nature and the laws of Nature, should have been, of all theories perhaps ever suggested by man, the most thoroughly misunderstood. There can be no doubt that many who recognise the real significance of the theory of natural development, who know that its influence is by no means limited to biological evolution, but has been felt in the far wider—the infinitely wide—field of cosmical evolution, have been pained by the thought that with the widening of the domain of development, the belief in a power working in and through all things seems to be set on one side in the name of universal evolution. It is this thought—this fear it may be called perhaps—which I propose to consider here. I shall endeavour to show that those who are perplexed by such doubts overlook the parallelism which exists between three lines along which men's thoughts have been carried an ever-increasing distance, until it has become obvious that two of them at least must be infinite—that the fear expressed by those who see with anxiety the progress of evolutionary doctrine implies a hope that one of these lines may be finite while the others are essentially infinite, and are accepted as such without fear or trouble.

It was a new thought in the time of Copernicus, that men had hitherto underrated the extent of the universe, and had overrated the importance of our earth. The globe, which had seemed the one fixed orb for whose benefit the heavenly bodies had all been made, was found to be but one member of a family of orbs circling round a globe much larger than any of them. Thus the earth lost at once her central position, her quality as *the* world (the sole abode of life), her fixity, her importance in respect of the supposed superiority of her dimensions. When Newton had finally established the Copernican theory,[1] the relative insignificance of the earth was demonstrated. The teachings of the telescope showed in turn the insignificance of the solar system. With every increase of light-gathering power the universe of stars grew larger and larger, even when as yet no scale had been obtained whereby to determine the distance separating star from star. With every improvement in the defining qualities and the measuring power of telescopes, the universe of stars grew larger and larger, independently of mere increase in number of stars; for though for a long time no measurement of star-distances could be effected, each failure with improved means to measure the distances of even the nearest stars showed that the scale of the stellar universe was larger than had before been imagined.

Larger and larger grew the universe, then, as men turned more and more powerful, more and more exact instruments to the survey of the heavens. When at length the distance of the nearest star was measured, and found to be more than twenty millions of millions of miles (more than three years' light-journey, though in each second light travels a distance exceeding nearly eight times the entire circuit of the earth), the number of stars was already known to exceed twenty millions. But more powerful telescopes have been made since. With every increase of telescopic power more stars come into view. With such a telescope as the great reflector of Parsonstown, at least a hundred millions of stars could be seen if every part of the stellar sphere could be scrutinised with that mighty telescopic eye.

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[1] It is worthy of notice that that theory could not be regarded as demonstrated until the law of attraction had been established. This law carries with it the disproof of the cycles and epicycles of the Ptolemaic system, because under the law of gravity bodies cannot move in such curves. Before the law was established, it was more probable that the planets all moved in simple curves, but not certain.

But what, after all, is this? Now that we know how minute a creature man is, how insignificant his largest works compared with the globe on which he lives, how this globe is but a point in the solar system, the solar system lost among countless millions of other suns with their attendant planets, how preposterous appears the thought that any instrument man can fashion can penetrate the real profundities of the universe! Seeing, as we do now, how utterly men's ideas of what the stars are fell short of the truth, and how more inadequate still were their conceptions of the real number of the stars when they trusted only to the natural eye, we should very ill have learned the lesson their errors teach us, if we in turn fell into the mistake of supposing that the telescopic eye can reveal more to us than the merest corner of the universe. Even of the universe of stars—that is, o the system of suns whereof our sun is a member—this may be said. But how unlikely, how incredible, indeed, is it that there is but one system of suns, but one galaxy? The star-clouds may not be outlying galaxies, as the Herschels supposed. It seems clear that they are but parts of our own galaxy, whose grandeur and complexity are far greater than had been supposed. But who can doubt that beyond the limits of our own galaxy, beyond spaces bearing probably something like the same proportion to the size of the galaxy that the interplanetary spaces bear to the size of our earth, come other galaxies, some like, some unlike, our own, some as large, some smaller, but many doubtless far larger than the glorious system of suns which appears infinite to our conceptions? 'As thus we tilt'—in imagination—'over an abysmal world, a mighty cry arises that systems more mysterious, worlds more billowy, other heights, other depths are coming, are nearing, are at hand.' Who can wonder if from these awful depths men have turned in weariness of soul, nay, almost in affright, as when the Alpine traveller, peering over some fog-enshrouded precipice, sees down, as the mist rolls past, to deeper and deeper abysses, until he is compelled to turn from the contemplation of the ever-growing depth? It is not simply the vast in which men have learned to believe, not mere immensity, but the mystery of absolute infinity. On all sides our island home is surrounded by a shoreless sea of space. So great has been the oppression of this mystery of infinity that men like Helmholtz, Clifford, and others have attempted, by rejecting the elementary conceptions of space, to show that there may be limits to space —not merely limits to occupied space, but limits to space itself—as though by closing his eyes, the traveller, oppressed by the vastness of the plain surface over which he voyaged, should endeavour to convince his mind that the end of his journey was close by him.

'Practically infinite,' as Huxley has expressed it, or absolutely infinite, space is (to all intents and purposes) infinite for us. But space and time are too intimately associated for us to imagine that space can be infinite and time finite; or that if occupied space grows, even under our survey, until we recognise that it is as infinite as space itself, time occupied by the occurrence of events (of whatever sort) can be otherwise than infinite too.

If we could reasonably doubt this we should yet find evidence as clear in this direction as with reference to space itself, though not so obvious to the senses. Everyone can understand the evidence of vast size presented by the universe as science is able to survey it; and everyone can see how the constant growth of the known universe points to the real universe as to all intents and purposes infinite. But not everyone can understand the evidence of the antiquity of the universe, or the certain promise which its features afford of a duration in the future which must be, like the duration of the universe in the past, practically infinite. But even to those who cannot see the force of the evidence on these points, it is obvious, so soon as the idea has once been presented—just as obvious as is the idea of infinite absolute space—that time itself, occupied by events or not so (if this could be imagined), must be absolutely infinite. The occurrence of events might perhaps be spoken of (not conceived very readily) as having an absolute beginning and proceeding onwards to an absolute end, this island of occupied time being lost in a shoreless ocean of void time; but none can reasonably *speak* even of a beginning or an ending of absolute time, far less conceive either thought.

Space, then, and time present themselves to our conceptions, and with the progress of research may be said to present themselves to our observation, as practically infinite. The earth, which has been displaced from her imagined central position in space, has been displaced equally from her imagined central position in time. The ocean of time which had been supposed bounded on one side by the beginning of this earth's history and on the other by the close of the earth's career, is seen to bear somewhat the same relation to the earth's duration that the Pacific Ocean bears to the tiniest islet of the least important Polynesian group.

Now in the days when the earth was thought to be central and all-important in space, central also and all-important in regard to time, a little knowledge—as limited and as imperfect—was possessed by men respecting the action of natural laws. They knew for example that animals, including man, pass through certain stages of development. They saw that the trees of the forest spring from seeds. They could trace further the growth and development of families of animals, the spread of vegetation over countries and continents; the formation, on the one hand, of tribes, nations, races, and species; on the other, of the various forms of vegetable development. But such knowledge, and all the ideas associated with such knowledge, were limited within the range of space and time over which alone in those days men were able to extend their survey. In fine, men recognised processes of development taking place upon the earth, and during her continuance as an inhabited world; they did not look outside either the region of space or the period of time which they had learned to regard as if they were in reality all space and all time.

In passing I may note that hitherto I have not heard that in the good old days—when the earth was the world and her life (very much under-estimated) all time—men who studied processes of development or evolution such as are plain and obvious to all were regarded as necessarily rejecting the belief in some power at the back of observed phenomena. On the contrary, so far as we can judge of the ideas of those days by what men said, it would seem to have been regarded as a wholesome thought, that under the operation of natural laws trees and animals, races and forests, grow from feeble beginnings till they fulfil all the functions of their several kinds. The more carefully such processes of development were considered, the more perfectly the laws of Nature seemed fitted to work out their seeming purpose, so much the more confidently did men regard those processes and laws as implying some plan or purpose; though also, it must be admitted, the nature of such plan or purpose seemed to the wiser sort the more inscrutable the more closely its workings were studied. 'Canst thou by searching find out God?' said one, who so far spoke truth, though he drew the wrong lesson from it; 'canst thou find out the Almighty unto perfection? It is as high as heaven; what canst thou do? deeper than hell; what canst thou know?' Another, who took a wiser view of Nature, yet in this spoke the same doctrine: 'Touching the Almighty, we cannot find Him out.'

In our day, with the extension of men's recognition of the vastness of space and time, there has come a widening also of their conceptions respecting the extent of the domain of natural law as well in time as in space.

And in the first place I would ask whether it is not naturally to be expected that this growth in our ideas respecting evolution should have followed (if it did not accompany) the growth of our conceptions of the extent and duration of the domain of evolution? It it had so chanced that neither research nor observation had availed to extend our recognition of the operation of natural laws—after Copernicus, Kepler, and Newton had established the true theory of the solar system—might not analogy alone have sufficed to convince men that the larger and longer-lasting universe shown them by science was governed by wider and more permanent laws than they had hitherto recognised?

But the Copernican theory had not been established without the demonstration of a law so general and far-reaching that when it had once been established no new recognition of law could be reasonably regarded as startling or unexpected. Newton had proved that the quality of gravity pertains to every particle of matter in all places and in every condition, and that it extends according to definite law to an infinite distance. At least, he had proved these properties so far as they *can* be proved. Every possible test had shown that the particles of solid, liquid, and vaporous matter equally possess (according to their mass) the quality of gravity. Every possible test had shown that not the external particles of suns and planets, or these in greater degree, but every particle, to the very centre of the largest and most massive globe, possesses in the same degree (according to its mass) this mysterious, all-pervading power. And lastly, every possible test applied to the movements of the heavenly bodies had shown that the force of gravity exerted thus by each particle diminishes as the square of the distance increases, but suffers no further diminution : so that the tiniest particle in the sun exerts, at least throughout the domain of the solar system, even to the orbit of Neptune, the force due to its mass and to the distance of any other particle on which its influence is exerted. In this inquiry the vast mass of the sun stands us in good stead. Were we only able to consider the attraction exerted by a single particle, or by a small mass at great distances, the smallness of the resulting attraction would foil any attempt to measure its amount with precision. But we can consider the total energy of the solar mass, exceeding 350,000 times the mass of the earth, at the distance of Neptune; in other words, we can examine the combined attractive force of a gathering of many millions of millions of particles, and having measured that, we can divide it in accordance with the known relative mass of the sun, and so ascertain whether each particle of the sun does its due work at the distance of Neptune. When we thus learn that there is not the slightest trace, even over that enormous range, of any diminution of energy beyond that belonging to the law of gravity as determined for a small distance (such as the moon's), we are justified in assuming that at a distance twice, thrice, many times as great as Neptune's the law of gravity holds unchanged. We have then a law whose action is to all intents and purposes universal; it operates in every particle of the universe, and it extends from particle to particle throughout the whole extent of the universe. Of a law such as this, if of any law at all, it might have been said that it seems to negative the action of a special Ruler. It was said of late respecting the general doctrine of development, that it sets the Almighty on one side in the name of universal evolution; with at least as much force it might have been said of the doctrine of attraction, that it sets the Almighty on one side in the name of universal gravitation.

We know indeed that such an objection was urged against Newton's doctrines in Newton's day and for many years after. Very probably if the theory of gravitation had not been established to demonstration by Newton and such followers as Laplace, Lagrange, and others, we might hear the objection even now (we hear it still among the ignorant, but of course it has entirely died out save with them). When the theory of universal gravitation became thoroughly established, it was found to be in perfect accordance with the idea of a universal lawgiver. Men presently began to wonder, indeed, how it could ever have been supposed that the laws of the universe must of necessity be limited in their range of action whether in space or in time.

Yet when the Newton of our own time advanced a theory which bears to biology (so far as is possible in matters so unlike) the same relation that the law of gravity bears to astronomy, a theory bringing animal and vegetable life under the domain of laws practically universal, an unreasoning fear possessed many lest this natural sequel of our growing knowledge of the universe should alter men's conceptions of the government of the universe. In space the universe was seen to be infinite, and in duration infinite ; a law infinitely wide in its operation had been found to govern all movements within the universe, yet the recognition of a new law, also indefinitely wide in its operation, instead of being regarded as natural and appropriate, was looked upon with disfavour and disapproval.

Note that we use the word indefinite, not infinite, in speaking of the operation of the law of biological evolution. The biologist cannot test the operation of this law so widely as the astronomer can test the operation of the law of gravity, for the simple reason that the biological law relates chiefly to time, while the astronomical law relates chiefly to space, and we can look with ever-increasing range of vision into depths of space which are practically infinite, while we cannot look with equal confidence into remote depths of past or future time. For the same reason that men even to this day accept more confidently the enlarged ideas of science with regard to space than the extended ideas with regard to time, which logically should be accepted with equal readiness, the theory of evolution must ever remain incomplete as compared with the theory of universal attraction. No one could urge with much effect, in these days, that perhaps beyond the range of the telescope the law of gravity which within that range (and far beyond the limits of the solar system [1]) we see in operation, may be replaced by some other law entirely different in its mode of action. But the opponent of the doctrine of biological evolution may, without much fear of effective reply, express the belief that before some definite epoch in the past, not evolution, but some other law or process, was at work in the fashioning of the various forms of animal and vegetable life. In dealing with space no one can reasonably say, that in whatever direction one may suppose a line extended, a limit must at length be reached beyond which we cannot, even in imagination, extend our survey. But in dealing with time it is not considered unreasonable, but, on the contrary, eminently reasonable, to say that far back as we may please to carry the process of evolution we must at length come to a beginning, before which there was not only no evolution of life, but no life to pass through processes of evolution.

Here, indeed, science assents in some degree to the objectors, if science may not be said to have given birth to the objection. Science has shown that with suitable care to remove or destroy all germs of life from a given space, no life will appear within that space—in other words, that, so far as scientific observation extends, the generation of life is never spontaneous. Equally science might assert that, so far as scientific observation extends, the generation of a system of orbs like the solar system does not occur spontaneously under any suitable test conditions. If a smile be excited by the thought of the vast difference of scale between any test conditions and the conditions under which our own solar system may have come into being, let it be noted that there must be a kindred difference between any experiments as to the possibility of spontaneous generation and the only conditions under which we can imagine spontaneous generation to have occurred. There is some difference, we submit, between a small flask with a few ounces of hay infusion, to which no air has been admitted, which has not been submitted to a number of life-destroying processes, and a young planet teeming with material vitality, still hot with its primeval fires, still palpitating from the throes which (during countless ages) had preceded and accompanied its birth. No experiment or observation man has ever made or can ever make, can suffice to show that the spontaneous generation of living forms *then* was either possible or impossible. But men may continue, if it gives them any comfort, to believe that just then the uniform action of law was interrupted, that just at that stage the mechanism of the universe was found to be imperfect.

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[1] Binary, triple, and multiple star-systems tell us of the operation of gravity in the star-depths; and so do the movements of stars in space, though not so obviously.

But while in this sense and to this degree the law of biological evolution differs from the law of universal attraction, the work of Darwin must yet be regarded as akin to that of Newton, in that it extends indefinitely our conceptions of the range of natural laws. As Newton showed men all the millions of families of worlds throughout the universe moving in accordance with the law of attraction, so Darwin has shown us all the myriads of races which have inhabited the earth brought into due relation to their surroundings by the operation of the law of evolution. And as the law of gravity was but a wider law, including such laws as Copernicus and Kepler had recognised, which in turn severally included many minor laws, so it should be noticed that the law of biological evolution includes all those minor laws of development which men had recognised for ages without entertaining the unreasonable thought that such laws necessarily implied the non-existence of a lawgiver.

To those alike who are pained and to those who rejoice at what they regard as the irreligious tendency of the doctrine of biological evolution, the same answer may be made: it is only when we try to create arbitrary limits of space or of time, and to set these as bounds to the operation of the laws of Nature, that any such tendency can be imagined. Those who have admitted the growth of a tree, a forest, or a flora, of an animal, a race, or a fauna, according to natural laws, have to acknowledge nothing new in kind, however different it may be in degree, in admitting that there is development on the larger scale as well as on the smaller, not even though they should have to admit that such development takes place throughout all space and all time. The difficulty in dealing with one thought is not greater than that which oppresses us in considering the other; both difficulties are overwhelming, both infinite. If we could evade the conception of the infinite in space or in time, we might be content to imagine limits to the operation of law. But we can neither evade the conception nor grasp it. As Pasteur has well said, quite recently—'When the question is asked, "What is there beyond the starry vault?" it is useless to answer, "Beyond lies unlimited space." When we ask what lies beyond the far-off time when what we see around us began to be, and what lies beyond the remote future when it will cease to exist, of what use the answer, "Beyond lie eternities of past and coming time"? Nobody understands these words. He who proclaims the existence of an Infinite —and nobody can evade it—asserts more of the supernatural in that affirmation than exists in all the miracles of all the religions; for the notion of the Infinite has the twofold character of being irresistible and incomprehensible. When this notion seizes on the mind, there is nothing left but to bend the knee. In that anxious moment all the springs of intellectual life threaten to snap, and one feels near being seized by the sublime madness of Pascal. Everywhere I see the inevitable expression of the Infinite in the world. By it the supernatural is seen in the depths of every heart.'

It is as thus viewed that the laws of development brought before us during the last quarter of a century—not as novelties, for in conception they are of vast antiquity, but new in the sense that now for the first time they are presented as proven—are so solemn and impressive when rightly understood. As the discoveries of astronomy were first steps towards infinite space, steps carrying us far enough upon the road to show that of necessity it *must* be infinite, as the study of the movements of the heavenly bodies tells us unmistakably of infinite time, so the recognition of development tells us that, as we might have anticipated, the domain of law is limitless alike in space and in time. With the angel in Richter's Dream, Science, in the doctrine of Everlasting Evolution, proclaims the solemn truth,—'End is there none to the universe of God; lo, also, there is no Beginning.'

THE VISTAS OF THE PAST.[1]

Many of those who follow with interest the teaching of science, but have not leisure to study carefully the methods and principles on which those teachings depend, are inquiring what new views are these according to which the moon was born of the earth many millions of years ago, and has been retreating ever since from the parent orb ; how those views are related to the nebular hypothesis of Laplace ; and what bearing they may have on astronomical and geological estimates of past eras in the earth's history. An eloquent lecture by the Astronomer Royal for Ireland has done much to increase the interest with which these questions are viewed ; indeed, it may be doubted whether many who are now inquiring about these matters had heard of them at all before Dr. Ball brought them before the attention of the audiences to whom his lecture has been addressed.

I propose to sketch the ideas resulting from the researches of Mr. George Darwin, noting how they are related to former views respecting the development of the solar system, and how they bear on certain other astronomical and geological theories. At the outset I may remark that I cannot altogether agree with the opinions expressed by Dr. Ball, and to some degree by Mr. Darwin, respecting the manner of the moon's birth ; but as to the general theory to which Mr. Darwin's researches have led there seems very little room for doubt or question.

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[1] This essay should be read in connection with that which follows. It will be found that they are in a sense supplemental to each other, portions which are fully treated in one being summarised in the other, and *vice versâ*.

In carrying back our thoughts to the past of the earth, our most trustworthy guide (though we must be careful in following even this guide) is evidence found in the study of processes actually taking place at the present time. For instance, we find that the earth is slowly cooling. We can, therefore, go back to a time when she was much hotter than she is at present; and though we may not be able to assume confidently that her temperature was ever so great as to cause every particle of her substance to be vaporised, we may infer even *that*, if other features actually existent seem readily explicable on such an assumption. Again, we find that the earth gathers in every year hundreds of millions of meteoric masses of greater or less weight, down to bodies weighing only a few grains; and we know from the orbits followed by the greater number of these that they belong to systems travelling around the sun on paths of such a nature as to forbid us to believe that they were originally expelled from the earth. Seeing, then, that the earth is gathering in materials from without, though now at a very slow rate, and seeing further that this process is of necessity one which takes place more and more slowly as time proceeds, we are justified in looking back to a time when it progressed far more quickly than at present, in considering that over the whole intervening period—many millions of years—it has been at work, and finally in inferring that no unimportant part of the earth's present mass has been derived in this way from meteoric aggregation.

Now, among other processes of change that are taking place in the earth and her dependent or associate orb, the moon, are two others, discovered in comparatively recent times, though not quite so recently as some might infer from Dr. Ball's account. About a quarter of a century ago Professor Adams, co-discoverer with Leverrier of the distant Neptune, announced that he had discovered an error in Laplace's discussion of the so-called acceleration of the moon, and that when this error was corrected the acceleration could not be entirely accounted for by the theory of gravitation. It was presently shown by the eminent astronomer Delaunay (not to be confounded for a moment with the Delaunay who has recently insisted on the inferiority of the weaker sex) that this unexplained part of the acceleration of the moon may be explained on the assumption that it is not the moon which is gaining, but the earth which is losing time ; in other words, that the great terrestrial clock, the rotating earth, by which we measure time, is not going at a uniform rate, but is gradually losing its rotation spin. Laplace's assertion that the earth's rate of rotation, so far as astronomy can measure, is appreciably constant, was based on his investigation of the moon's so-called acceleration. Supposing that no part of this change remained unexplained, when solar and planetary perturbations of the moon were taken into account, he naturally inferred that the great terrestrial timepiece is keeping most perfect time. Finding, on the contrary, that a part of the acceleration does remain unexplained, we are justified in assuming, as at least a possible interpretation of the excess of acceleration, that our chief timepiece is losing time. Delaunay pointed to the tides as a probable and sufficient cause of this change—the great tidal wave carried, not bodily, but still swayingly, against the direction of rotation, checking the earth's rotation spin slowly but 'exceeding surely.'

Next, it was shown that, accompanying this change, there must be a gradual loss of lunar motion, accompanied by a gradual recession of the moon.[1]

In the essay which follows, on The Birth of the Moon, I describe more at length these two processes of change.

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[1] This may seem inconsistent with what we said above about the lunar acceleration which astronomers have endeavoured to explain. But this acceleration is one of the temporary changes which the moon's motion undergoes. It alternates with a similarly temporary retardation, in periods of great length indeed, but not to be compared with the enormous time-intervals which we are considering.

Here, for the present, let it suffice to note that astronomy recognises them as taking place, and that they therefore are among the processes which we may carry back in imagination to a very remote past, in order that we may recognise what probably was the initial condition—at any rate, a very early condition—of the orbs in which they are taking place.

Of course it is an obvious thought that if the moon is thus receding now, and has been receding in the past, she will one day part company with the earth altogether, and that she was at one time quite close to the earth, and even a part of the earth's mass. Considering, also, the change in the earth's rotation period, and carrying our thoughts as far back into the vistas of the past for this change as for the other, we see a time when the earth was rotating so fast that its equatorial parts were barely restrained by gravity from yielding to the tremendous resulting centrifugal tendency. A simple calculation shows that if the earth rotated once in about one hour and a third, retaining its shape unchanged (which last it could not do unless very much more rigid than it is), a body at the equator would be absolutely weightless. But a much slower rate of rotation than this would suffice to break off the equatorial regions. If the earth rotated once in about three hours, the equator would increase its distance from the polar axis, the centrifugal tendency (the rate of rotation continuing) would be greater and the surface gravity less, and the material of the equatorial surface parts would be separated from the rest of the earth's substance.

Dr. Ball follows Mr. Darwin in taking about this rotation rate—one spin in three hours—as that existing when the moon's mass separated from the earth. If we assume the earth at that stage of her existence to have been, apart from centrifugal effect, of the same volume and mass as at present, her substance possibly liquid, but not in great part vaporous, this estimate would be justified. But it appears to me we must not overlook the probability that the separation of the moon from the earth took place when a large part of the earth's mass continued vaporous through intensity of heat. If that were so, the earth's volume would then have been much greater than at present, even though her mass may have been, as it probably was, much smaller. What we see now in the giant planets, long after the moon-generating part of their career, seems to confirm this view, which *à priori* reasoning renders probable. We have also to take into account the smaller mass of the earth at that remote period, before those many millions of years throughout which the earth has been gathering year by year hundreds of millions of meteoric masses.

Now, with a larger and less dense orb, a slower rotation rate—probably a rotation rate very much slower—would have sufficed to cause the earth to part with matter from its equatorial regions, where, of course, the centrifugal tendencies resulting from over-rapid rotation would be most pronounced.

I have been in the habit, during the last ten years, of pointing out when lecturing on the moon that she probably had her origin as part of the vaporous or partly vaporous mass whence the earth also was formed, and that to this origin she owed the peculiar rotational motion which keeps the same face ever directed towards the earth. I can see nothing in Mr. Darwin's researches which should lead us to forsake this, the most natural interpretation of the moon's origin; on the contrary, the vast duration of the past periods necessary for the increase of the moon's distance from actual contact with the earth to her present orbit, and for the increase of the terrestrial day from three hours to twenty-four, suffices of itself to assure us that the earth at that remote time must have been in great part vaporous. The giant planets also, as I have already hinted, tell the same story; for though they have thrown off their moons—Saturn, perhaps, has not quite finished the work—they are still, as we can see from their small density and their aspect, in great part vaporous. When they were beginning the work of moon-formation, many tens of millions of years ago, they were, we may be sure, still hotter, and therefore a much larger portion of their mass was vaporous.

But it is the manner of the moon's birth, as suggested by Mr. Darwin (Dr. Ball accepting the suggestion as probably sound), which seems to me least likely to accord with the probable manner of the moon's generation, and also to correspond least with *à posteriori* evidence.

Mr. Darwin pictures the earth rotating once in three hours, with a double tidal wave (a wave affecting the fluid substance of her entire mass), raised by solar action. Such a wave, synchronising with what may be called the pulsation period of the earth (with the dimensions she then had), would get higher and higher, just as a pendulum receiving a succession of minute but well-timed impulses swings farther and farther, until at length cohesion would no longer be possible, and the mass out of which the moon was one day to be formed was thrown off. The considerations I have indicated above would not affect this reasoning; they would only modify our views as to the size and condition of the earth when the moon's mass was thus liberated, and therefore as to the rate of the earth's rotation spin at the time, and the period of the moon's first free revolution. But there is a more important consideration, now to be taken into account, which forbids us, I think, to believe that the moon's mass was thus thrown off, as it were, by a single effort. The monstrous tidal pulsation which would undoubtedly take place under the conditions described, would inevitably lead to the throwing off of a small mass long before it had attained swing enough, so to speak, to throw off such a mass as the moon's—one eighty-first part of the entire mass of the earth. Most probably, too, the crests of each tidal wave would throw off a mass of matter at about the same time, forming, for the time, two small moons instead of one large one. Still more probably, in my opinion, the crest of each wave would scatter cosmic spray rather than a single great globular mass. After each wave had thus swollen and eventually burst into spray, it would gradually subside for a while, the solar tidal impulses no longer quite synchronising with the earth's tidal pulsation; but presently the waves would begin to grow again, would flow larger and larger, until again a flight of small masses would be flung from the summit of each. Again and again the process would be repeated, until at length the earth's constantly changing rotation rate would cause the sun's tidal action no longer to synchronise with the earth's pulsation period. Then, and only then, the earth would cease to throw off cosmical spray.

Now what would be the condition of the matter thus thrown off, and what its subsequent behaviour? Each particle, each globule of molten matter, would behave just as the moon, according to the theory we are considering, has actually behaved. It would begin from the first moment of its separate existence to retreat slowly from the earth. Long before the tidal wave had again grown sufficiently high to throw off spray, the spray last thrown off would have passed beyond its reach. Again, each of the tiny globules thus thrown off from the earth would at first travel nearly in the plane of the earth's equator (later influences would modify this relation considerably). Thrown off with slightly varying directions and degrees of velocity, the bodies expelled on opposite sides at one of these earth-spasms, would before long have spread themselves all around the earth, some gaining on the main body, others losing. Probably before the next flights of cosmical spray left the earth, the bodies last thrown off would form a tolerably uniform very narrow ring around the earth.

This process would have continued between certain definite epochs—the first being the time when the earth's rotation began to approach to synchronism with her pulsation period,[1] the last being the time when there began to be no sufficient approach to synchronism (in the mid-interval only would there have been perfect synchronism). This period must have lasted for a very long time, probably for millions of years. When it was over, what was the condition of the matter which had been thrown off from the earth's mass? Manifestly it must have formed at that time a series of close concentric rings of tiny satellites. Probably the rings were so close that, though each was very narrow, they formed a continuous flat and rather broad ring. But, whether this were so or not, it is certain that the outermost and innermost ring of the series would form the boundary circles of a flat and rather broad ring-system of small bodies, closely resembling in appearance (as seen from a great distance) the Saturnian ring-system, and having a real structure precisely like that which the researches of Benjamin Peirce and the Bonds in America, of Clerk Maxwell and others in this country, have proved that the Saturnian ring-system actually has.

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[1] That is the period of vibration of her mass after any impulse (affecting the whole earth) had been received from without. The earth would as certainly have had such a pulsation period as the vibrating substance of a bell has.

It seems to me, on the one hand, so clear that the process suggested (with great plausibility) by Mr. Darwin and Dr. Ball must really have taken place in such a manner as to produce a ring such as I have described; and, on the other hand, it is so certain that the Saturnian ring-system is of this nature, that I feel persuaded that we have here been led—by paths along two lines of research, each of great difficulty, apparently tending in very different directions—to the explanation of the mystery of Saturn's rings, and of the much deeper mystery of the origin of worlds and moons. Sixteen years ago, in the preface to my treatise on 'Saturn and its System' (my first work), I pointed out that probably in the study of the Saturnian rings we might find an interpretation of the manner in which the solar system itself had been developed. My prediction, if such it can be called, has not been exactly fulfilled, though the relationship I indicated between the two problems has been confirmed. For, instead of the study of the Saturnian ring-system having thrown light (except reflected light) on the origin of worlds and moons, it would seem as though the study of the origin of the moon had thrown light on the Saturnian rings.

Be this as it may, there can be very little question, I believe, that the moon was not formed at a single effort, as Dr. Ball has suggested, but that a series of rings was first formed, constituting a single flat ring-system. The formation of the moon from such a system of rings would result from the gradual process by which the number of the minute bodies forming the ring-system would be reduced by collisions. If the ring-system was (as seems probable) immersed at the beginning, and for a long time, in the vaporous outskirts of the earth, this process would be less slow than it otherwise would have been. Satellite after satellite would coalesce with neighbouring satellites; probably, centres of aggregation would be formed, which would absorb wandering satellites in the ring-system still more effectively. Every combination of the kind, by changing the period of revolution of the mass thus formed (for at every collision there would be a loss of *vis viva*), would tend to hasten the change of the ring-system into a single orb. It is no new idea that such a process as this took place, no mere attempt to reconcile new results with views previously entertained. The occurrence of such changes as I have here described was indicated by me sixteen years ago, in my treatise on Saturn (p. 126), and it was there shown that changes in the appearance of the rings, and probably the recent development of the inner dark ring, may be due to processes of this kind —collisions among the satellites, and consequent loss of *vis viva* by the entire system.

The formation of the moon, whether in this manner, which appears to me much the more probable, or as a single catastrophic event, must have occurred at so remote a period that the earth's rotation (carrying back over this enormous interval of time the process of retardation, which has certainly been in progress) must, when the moon was first formed, have been much more rapid than at present. The moon's period of revolution, also, must have been very much shorter than it now is. From and after that era, the processes of change must have been those which Mr. Darwin has described, and which Dr. Ball has pictured (with colouring in some parts perhaps *tant soit peu* exaggerated). We have no occasion to explain, as the latter *savant* does, how the earth's frame recovered from the shock of the moon's genesis, or how the scar left on her then plastic surface, where the moon's mass had left her, was presently healed by the 'gentle ministrations' of the mutual attraction of the particles forming her substance ;[1] for no such scar would ever, according to our view, have marred the fair surface of the earth. But subsequent changes would have been the same in whichever of these two ways—the sudden or the gradual—we suppose the moon to have been formed.

According to either view, it is by no means clear that the moon's rotation period would have been the same as her period of revolution around the earth, as is now the case. But it is certain, that from the beginning of her existence as an independent orb, the moon must have been at work in raising a tidal wave, and at first far more actively even than now. Not only would she have raised a higher wave, because nearer to the earth, even had the earth been then what it is now ; but since the earth must then have been in great part fluid, the moon would from the beginning do what the sun had for countless ages been doing—she would raise, like him, a tidal wave affecting the whole fluid substance of the earth ; and, owing to her much greater proximity, the tidal wave she thus raised must of necessity have been very much greater than that raised by the sun. This tidal wave, like that now raised by the moon, would retard the earth's rotational spin, and much more effectively. The retardation of the earth's spin would then, as now, be accompanied by a gradual retardation of the moon's motion, and recession of the moon from the earth. And while these changes were taking place, the earth, by her attraction on the then fluid mass of the moon, would be producing similar effects. The moon (supposing her then to have rotated in less time than she occupied in revolving round the earth) would be acted upon tidally by the earth. A mighty wave of fluid or at least plastic matter would circle round the moon in a direction contrary to that in which she was rotating; she would, therefore, gradually lose her rotational spin, just as the earth was losing hers, only at a more rapid rate. The reaction corresponding to this action would be, in the earth's case, as in the moon's, shown by increased distance. In other words, the earth's rotation and the moon's rotation would both be reduced in rate, the moon's the more rapidly, and both changes would combine reactionally in increasing the distance separating the two bodies.

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[1] 'By these gentle ministrations,' says Dr. Ball, 'the wound on the earth would soon be healed. In the lapse of time, the earth would become as whole as ever, and at last it would not retain even a scar to testify to the mighty catastrophe.'

Only one of these processes is now going on—the moon's action is diminishing the earth's rotational spin (and the moon's distance is therefore increasing by reaction), the earth's action is not diminishing the rotational spin of the moon. The reason why the latter action no longer produces any effect is that it has done its work, it has no longer anything to work upon. The moon's rotation now synchronises with her revolution around the earth, there is no tidal wave (there could be none if the moon's entire surface were covered by ocean, or even if the moon's entire mass were fluid), and therefore there is no loss of rotational spin. I have said the earth no longer has any work to do so far as modifying the moon's rotation is concerned. This is nearly true, but not quite. The earth has still some work to do, in preventing the rotation rate of the moon from diminishing, as it would otherwise tend to do, under the sun's action. If the earth were suddenly destroyed, or rather removed entirely away from the solar system, the moon would continue to travel around the sun, in a path very little changed from that which she at present follows, and, by such wave-motion as the sun can produce in the moon's mass, her would tend slowly to diminish her rate of rotation. The neighbourhood of the earth prevents any such change from occurring, and would do so, even if the sun could raise a large tidal wave in deep lunar seas or in the moon's entire mass. It will be seen presently that this is a consideration of some importance. There is also some work for the earth to do—though it is but slight—in diminishing the moon's rate of rotation so as to correspond exactly with the slow gradual increase in her period of revolution. Students of the moon could well wish this were otherwise, so that the farther side of the moon, which we never see, might come, however slowly, into our ken.

The earth, then, acting on the moon caused the moon to adopt that mode of motion which we recognise in her, turning once on her axis while she revolves once around the earth. In this peculiarity of the moon's motion we recognise one piece of evidence, which of itself is absolutely convincing, as to the vastness of the time-intervals which have elapsed since the moon first began her independent existence. Whatever the moon's original rotation period may have been it was certainly very much shorter than her present rotation period. If we suppose it identical originally with her period of revolution there would have been an enormous amount of work for the earth to do in gradually reducing the period to its present value—both periods, in point of fact, simultaneously. We have, then, to carry back the earth's history so far that, independently of all other evidence to that effect, we find ourselves forced to accept the conclusion that, at the beginning of the separate existence of earth and moon, our earth was a globe rotating much more rapidly than at present and much nearer to the moon.

And here the question arises whether we can find in this inference any explanation of the undoubted discrepancy between the teachings of geology and those of astronomy as to the earth's age. On the one hand the study of the earth's crust tells us of one hundred millions of years at the very least during which the earth has been the scene of changes such as are now in progress, chiefly—one may say, altogether—under solar influence. On the other hand, regarding the sun's emission of heat as resulting, in the main, from the contraction of his mass, we find that, assuming his density uniform, or nearly so, the contraction of his mass to its present dimensions, even from a former infinite extension, would have resulted only in generating as much heat as would last, at the present rate of emission, about twenty millions of years. We do not gain by supposing the rate of emission less in former ages of the earth, for then, the rate of solar work on the earth being less, the length of time necessary to complete the work which has actually been done would have been proportionately greater.

The difficulty is very serious. Dr. Croll, who was one of the first to call attention to it, suggested the explanation, which I take to be inconceivable, that our sun was generated by the collision of several orbs which had been rushing through space with enormous velocity, and that his supply of heat represents the energy of those rushing suns, as well as that resulting from compression. My own solution of the difficulty is one which is confirmed by other researches, including an important investigation by Mr. G. Darwin, that the sun is not of nearly uniform density throughout his apparent globe, but that he is enormously compressed towards the centre, and that, in point of fact, the surface we see lies very far above the real surface of the sun.

Dr. Ball believes that in the former proximity of the moon we may find a complete answer to the enigma. In the primitive oceans, he says, the moon raised tides as she does now, but when she was nearer the tides were much higher than at present. For instance, when the moon's distance was but forty thousand miles, or roughly, a sixth of her present distance, her tide-raising power was not six times, but two hundred and sixteen (six times six times six) times greater than at present. So far Dr. Ball's reasoning is sound; but I cannot follow him in saying that, therefore, the tides would have been two hundred and sixteen times as high as at present. (There is no such simple relation as this between tide-producing energy and the height of the tidal wave.) Still, we may admit that the tides were very much higher then than now.

> 'These mighty tides,' says Dr. Ball, 'are the gift which astronomers have now made to the working machinery of the geologist. They constitute an engine of terrific power to aid in the great work of geology. What would the puny efforts of water in other ways accomplish when compared with the majestic tides and the great currents they produce? In the great primæval tides will probably be found the explanation of what has long been a reproach to geology. The early palæozoic rocks form a stupendous mass of ocean-made beds, which, according to Professor Williamson, are twenty miles thick up to the top of the Silurian beds. It has long been a difficulty to conceive how such a gigantic quantity of material could have been ground up and deposited at the bottom of the sea. The geologists said, "The rivers and other agents of the present day will do it if you give them time enough." But unfortunately, the mathematicians and the natural philosophers would not give them time enough. The mathematicians had other reasons for believing that the earth could not have been so old as the geologists demanded. Now, however, the mathematicians have discovered the new and stupendous tidal grinding engine. With this powerful aid the geologists can get through their work in a reasonable period of time, and the geologists and the mathematicians may be reconciled.'

I am disposed to doubt seriously whether mathematicians and astronomers have done more than to somewhat alleviate the pressure of the difficulty we are considering. That they have subtracted somewhat from the work which had formerly been assigned to the sun must be admitted. We need not inquire what was the former height of the tides, or discuss the action of the tidal wave in detail. If we consider only that the tidal wave, according to the very theory we are considering, has, by its reaction against the earth, reduced the earth's rotation-spin from a rate of one rotation in perhaps not more than three hours, certainly not more than six, to one rotation only in twenty-four hours, we see that the work done on the earth's crust must have been enormous. It represents the friction-products, so to speak, of all that work. The wonder might rather be that the ocean-made beds are not much thicker than they are, than that they are so thick. But here is our difficulty returning to us in another form. Is it clear that the beds considered by Dr. Ball were not made subsequently to the time when the moon was at the comparatively small distance he mentions? Can we for a moment imagine that the tremendous work of checking the earth's rotation-spin to less than a quarter of what it was, has only left such traces as these? Must not that work have been done while still the greater part of the earth's mass was fluid, and the water tidal wave have begun its work long after? Geologists have other reasons than the thick ocean-made strata for their belief in the vast periods of time which form the great difficulty of the problem. There is the evidence derived from the study of organic matter, the evidence derived from the remains of once-living creatures—animal and vegetable. The moon might have raised a tidal wave as high as Chimborazo without hastening the progress of what may be called the development of the earth—nay, she would very seriously have checked this progress. It may be doubted, even, whether life, belonging to any save the lower forms, could have existed during the time when such tidal waves as Dr. Ball pictures careered round the swiftly rotating globe.

It remains to be noticed that, though the day will continually increase as the moon recedes, and, *vice versâ*, the length of the month, measured in days, attained long since its maximum. It was then—some millions of years ago—about twenty-nine days long, and is now but twenty-seven and one-third days, as days are now. As the moon recedes, the lunar month—which is also the moon's day—will contain fewer and fewer of our terrestrial days. For our days grow longer, now, at a greater rate than the lunar month increases. Our day will continue to grow longer and longer as the moon recedes. In one hundred and fifty millions of years, or thereabouts, our day will be about one thousand four hundred of our present hours long; this period, also, will then be that in which the moon circles around the earth—about fifty-eight and one-third of our present days. Dr. Ball goes on to consider how the sun would affect this state of things. There would be a tide raised by the sun on the earth after the moon had ceased to raise any tide (the earth's rotation exactly synchronising with the moon's revolution); and, as a result of this, Dr. Ball says, that the earth would begin to rotate in a longer time than the moon circles round her. It appears to me that the moon's action would check any tendency of this sort, just as the earth's action on the moon has, as we know, prevented the moon from rotating in a longer period than that of her revolution round the earth. The state of compromise with a moon circling once in one thousand four hundred hours round the earth rotating in the same time, the moon also so rotating, would be, I believe, a state of stable equilibrium. It is not a very pleasant future to look forward to. Fortunately it is very remote.

THE BIRTH OF THE MOON.

FOURTEEN years have passed since, in the first series of my 'Light Science for Leisure Hours,' I discussed the change which the tidal wave is slowly but surely producing in the length of the day. Certain researches, which had then recently been made into the moon's motions, had shown astronomers that there must be some force at work retarding the earth in her rotational spin. 'In this difficulty,' I wrote at that time, 'we are not left wholly without resource.' We are not only able, I showed, to say that the discrepancy between the moon's motions and theory is due to a gradual retardation of the earth's rotation-movement, but we are able to place our finger on a very sufficient cause for such a retardation. One of the most firmly established principles of modern science is this, that where work is done, force is in some way or other expended. The doing of work may show itself in a variety of ways—in the generation of heat, in the production of light, in the raising of weights, and so on; but in every case an equivalent force must be expended. If the brakes are applied to a train in motion, intense heat is generated in the substance of the brake. Now, the force employed by the brakesman is not equivalent to the heat generated. Where then is the balance of force expended? We all know that the train's motion is retarded, and this loss of motion represents the requisite expenditure of force. 'Now,' I asked, 'is there any process in nature resembling, in however remote a degree, the application of a brake to check the earth's rotation?' 'There is,' was the answer; 'the tidal wave, which sweeps twice a day round the earth, travels in a direction contrary to the earth's motion of rotation. That this wave "does work" no one can doubt who has watched its effects. The mere rise and fall in open ocean may not be strikingly indicative of "work done;" but when we see the behaviour of the tidal wave in narrow channels, when we see heavily-laden ships swept steadily up our tidal rivers, we cannot but recognise the expenditure of force. Now, where does this force come from? Motion being the great "force-measurer," what motion suffers that the tides may work? We may securely reply, that the only motion which can supply the requisite force is the earth's motion of rotation. Therefore, it is no mere fancy, but a matter of absolute certainty, that, though slowly, still very surely, our terrestrial globe is losing its rotation-movement.'

The discovery on which this conclusion was based has borne notable fruit in recent times. The change which the moon's motion was shown to undergo and the change which affects the earth's rotation were proved to be alike important. They are processes actually taking place, and scarcely any process which takes place now fails, when rightly understood, to throw light on changes which have taken place in the past. Very notably has this proved to be the case in the present instance.

Let us first briefly sketch the original discovery. Its history, carefully studied, affords an excellent lesson in showing how important it is for science that even the slightest apparent departure from theory should be noted, and that, when noted, it should be thoroughly investigated.

When the theory of gravitation was as yet in its infancy, in the lifetime, indeed, of its great author, a discovery was made which threatened to invalidate it. Halley, the first of Newton's followers, found that, when the eclipses of the sun, which are recorded in ancient annals, are examined in detail, the lunar motions necessary to explain them are different from those of the moon in our own time; that, in fact, she must have moved more slowly in past ages than she does at present.

Ninety years passed before any satisfactory solution was offered of the remarkable circumstance thus detected. Then the great mathematician Laplace showed how the moon's movements are in reality being hastened on account of a change which is taking place in the form of the earth's orbit. The moon travels round the earth[1] under the action of terrestrial attraction; but the sun, though much more remote than the earth, largely influences the moon's motion. Indeed, the sun is, in reality, the moon's chief guiding power. In regard, however, to her motion considered in reference to the earth, the sun has only a subordinate influence. This influence tends on the whole to diminish the earth's power on the moon, so that the latter travels in a wider orbit and more slowly than she would but for the sun. The nearer the earth to the sun, the greater is the sun's power to diminish the earth's sway, and the more slowly does the moon move. In December and January, for example, the lunar month is longer than in June and July, the earth being nearest to the sun on about January 1 and farthest from him on about July 1. Now, the eccentricity of the earth's orbit is at present, and has been for many years, diminishing, her path becoming more and more nearly circular (though it will never become actually circular, the eccentricity attaining a minimum after a certain long period of time, and then gradually increasing). As the longer axis of the path remains unchanged, it follows that the area enclosed by the earth's path is gradually increasing, so that on the whole the sun's perturbing influence is diminishing. Since this influence acts to increase the moon's distance and diminish her rate of motion, it follows that, as the influence diminishes, the moon's rate of motion increases. As the eccentricity of the earth's orbit has been diminishing throughout all the time over which astronomical records extend, it is clear that the hastening of the moon's motion discovered by Halley may find its explanation in this change.

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[1] In reality the moon travels round the sun, and is in that motion largely perturbed by the earth. But considered with reference to the earth as centre, the moon travels round the earth, and is in such motion perturbed by the sun.

Laplace supposed that this was actually the case. His investigation of the so-called acceleration seemed to be no less exact than profound. The calculated acceleration agreed so closely with what observation appeared to indicate, that science was supposed to have achieved a great triumph, and the law of gravitation, which for a time had seemed shaken (at least, it seemed as though some hitherto unknown forces must have been at work), was placed on a sounder basis than ever.

But our great astronomer and mathematician Adams, having re-examined this question twenty years or so ago, discovered a notable flaw in Laplace's reasoning. He found that disturbances which Laplace had supposed he might neglect, were in reality important. Laplace had considered slow variation in the sun's action in diminishing the earth's pull on the moon, but he had regarded as probably insensible the slow variation taking place all the time in the sun's direct action on the moon. When all the disturbing forces were duly taken into account, it was found that the calculated acceleration of the moon's motion amounts to only about half what Laplace had made it. The observed acceleration then, which was very satisfactorily explained so long as Laplace's results were accepted, was found to be but half accounted for. The other half had still to be explained.

It was then that Delaunay and others advanced the theory that perhaps the so-called acceleration of the moon may in part be apparent only. We measure the movements of the celestial bodies by our earth's, taking as our unit of time-measurement the period in which our earth rotates once on her axis, or what is called a sidereal day (the solar day is four minutes longer, and changing all the time). This had been regarded as absolutely constant. It is the basis not only of celestial motion-measurements, but of our entire system of weights and measures. But Delaunay suggested that our great terrestrial timepiece may be losing and may be running slow—not only losing time day by day as compared with the time she gave thousands of years ago, but losing rate, so as to go more and more slowly as time goes on.

But let us see on what grounds all these investigations had proceeded, and what was the real observed peculiarity of the moon's motion about which so much inquiry had been made. The moon is apparently moving more quickly now than she was 2,000 years ago; but suppose astronomers timed her now, and that, with all their acquired knowledge respecting the acceleration due to the cause indicated by Laplace, they calculated her position day by day among the stars for the next thousand years. As time went on, how much would the moon seem to gain by that remaining part of the acceleration which has not been accounted for? Well, at the end of the thousand years, she would not quite have gained half her own apparent diameter. Twelve hundred years would have to pass before her centre would be where, according to calculation, her forward edge should have been. In reality, it would be not the moon which would have gained so much, but the earth which would have lost so much. In twelve hundred years the earth's spin would be less than it should be if there were no change by the amount of rotation corresponding to what would carry a line from the earth's centre over half the breadth of the moon's face. That would be about a quarter of a degree, or, roughly, about a 1,400th part of the entire circuit of the heavens. As the moon completes the circuit of the heavens in twenty-seven days and a third, the difference in time would therefore be, roughly, about a 1,400th part of this, or some twenty-eight minutes. Looking back instead of looking forward, let us consider what the earth, regarded as a timepiece, has lost during the two thousand years which have elapsed since the earliest eclipses of which we have exact records. 'Suppose,' I wrote fourteen years ago, 'that the earth was then timed and rated, how much has she lost, and what is her "rate-error"? She has lost in that interval nearly an hour and a quarter, and she is losing now at the rate of one second in twelve weeks. In other words, the length of a day is now more by about one eighty-fourth part of a second than it was two thousand years ago.'

Such was the minute, one might have supposed the inappreciable, change in which was to be found the secret of some of the most important cosmical phenomena. In this slow change, minute in itself, and detected by its action in a period of time which, though long compared with man's life, is the merest nothing compared with the eras of a world's lifetime, science was to find an explanation of changes in the past so stupendous that the mind can hardly realise them; the indication of changes so great in the future, that when they have occurred life on this earth will no longer be possible (at any rate, to creatures resembling any now existing on the earth's surface).

The result thus far noted is that owing to the tidal action of the moon the length of the day is increasing. We may look forward into the remote future for a time when the day will be twice, or thrice, or four times as long as at present. Or we may carry back into remote depths of past time the change thus taking place, and recognise an epoch when the earth rotated in half the time, another when she rotated in a quarter of the time, in which she now rotates. But before we do this we have to ask what becomes of the rotational energy which is thus being lost; for whatever effects we find accompanying this change must also be traced forwards and back.

Here is our spinning earth losing her spin in consequence of the moon's action. It may be that the loss of energy thus indicated is entirely compensated by the heat resulting from the frictional action on which the diminution of the rotation rate depends. In that case we need not look elsewhere for any counter effects. But we may at the outset see that some of the results of tidal action on the earth must produce counter-effects outside the earth. If we imagine the great tidal waves set in motion once for all around the earth, and gradually retarding by their frictional action the earth's rotation, we know that besides the loss of rotation, and besides the generation of heat, there would be another observable effect—viz., the gradual dying out of the tidal wave. Now, the tidal wave being maintained from without by the lunar action (we may leave out of consideration for the moment the sun-raised solar part of the tidal wave), we see that one of the counter-effects which, but for the external action, would accompany the frictional retardation of the earth's rotation is not taking place. We have to seek for some other counter effect. As there is none on the earth, we may take it for granted that some such effect exists in the motions of the moon, the orb which is concerned with the earth and the sea in the tide-raising action.

Now, analysis of the matter by strictest mathematical investigation, in the able hands of Mr. George Darwin, Fellow of Trinity College, Cambridge (son of the great Charles Darwin), has shown that this effect must be a gradual diminution in the rate of the moon's motion, accompanied by an equally gradual increase in the moon's distance. The reader must not expect that the reasoning can be made clear to him in such a paper as the present. All these questions of the action and interaction of rotating and revolving bodies require for their discussion the profoundest mathematical knowledge, as well as that which such knowledge of itself indeed implies, the keenest mathematical insight. When I notice that even so skilful a mathematician as Sir George Airy, in a mathematical investigation especially relating to the moon's tide-raising action in diminishing the earth's rotation-spin, had actually announced that there is no such influence, only at the last moment detecting, amongst the complex formulæ involved, the presence of terms which, duly developed, indicate such action,[1] it will be seen how utterly inadequate must be the discussion of the matter on ordinary mechanical principles to indicate to the general reader the necessity of the change in the moon's motion and distance demonstrated by Mr. Darwin.

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[1] Another remarkable illustration of the difficulty of all such investigations is to be found in the rejection by Leverrier and Ponte-

Mathematical analysis shows unmistakably, however, that the moon's distance must continue to increase while the earth's rotational motion continues to diminish. These processes take place continuously, though not at an unvarying rate. We can carry them back to a beginning, and forwards to an end. Nor does science know of any circumstance in the past or in the future of the earth which should prevent us from carrying each process to its extreme limit either way; in other words, there is no scientific reason for believing that the earth and moon began their existence at some one of the stages to and through which we can trace the processes of change in the past, or that the continuance of these processes in the future will be suddenly brought to an end before they have completed their work. As to the past, indeed, science has very strong evidence to show that these processes started into action at the very beginning to which they can theoretically be traced. Just as from the study of a tree, an experienced gardener can tell that it grew from the seed, and not from a slip or cutting, so the astronomer and geologist can infer, from the evidence presented by the earth and moon, that they started into separate existence from the primordial planetary state,[1] and not more fully fashioned as companion planets.

Our retrospective view into the vistas of long past time encounters no obscuring mist till we reach a time when earth and moon formed one mass. How long ago this may

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coulant of the results obtained by Adams, as mentioned above, Pontecoulant even denouncing Adams's method of treating the subject as analytical legerdemain (*supercherie analytique*).

[1] We mean by the words 'primordial planetary state' to distinguish the earliest condition of planets whereof we have scientific evidence from the absolutely primordial condition of matter, of which science knows nothing.

have been we do not know. It must have been more than fifty million years ago, if we can at all trust the astronomical and geological evidence. Probably it was a good deal more than 100 million years ago. It may have been farther back still. We see it in the dim vista of the past, but it looms in such sort that we cannot estimate its real distance. We are more apt to think it nearer than farther than it really is. I will not follow Dr. Ball in citing apt illustrations of similar doubts, even in matters historical. It must be evident that when we have to deal with processes operating on so large a scale, and requiring such enormous periods of time, there can be nothing in our experience enabling us even to approximate to the exact time intervals. We must be content to say that they are measurable by tens of millions of years, but how many such tens of millions of years they include we cannot tell.

The critical epoch to which we look back may be regarded as the time of the moon's birth. Diminishing in imagination the period of the earth's rotation, we find gravity amply competent to keep the earth's mass together against the resulting centrifugal tendencies as the day passes down to half a day, to a quarter of a day, and even to a much less period. But it is evident there must be a limit to this. Suppose the earth were rotating at such a rate, for instance, that gravity vanished at the surface of the equator. Then it might at first seem as though, with such a rate of rotation, the equatorial parts would simply remain as they are, since gravity being just balanced by, and just balancing, centrifugal force, there would be no tendency in the equatorial parts to separate from the rest of the earth. But a little consideration will show that, on the contrary, with this rate of rotation, the earth must inevitably fly to pieces like a grindstone set in too rapid rotation. For there being no pressure at the equator, and very little pressure throughout the neighbourhood of the entire equatorial plane of the earth, whereas near the poles and along the neighbourhood of the polar axis there would be great pressure, and elsewhere a pressure increasing as the polar axis was approached, it follows inevitably that these pressures not being balanced in the neighbourhood of the equatorial regions, these must be forced outwards. At an increased distance from the polar axis the rotation of these masses in the short period mentioned would produce a centrifugal tendency exceeding the force of gravity. They must, therefore, either separate from the rest of the earth, and not attain the same rapid rotation as the rest, or, attaining it, they must be separated by being thrown outwards from the centre. In either case there would be a separation of the equatorial masses from the rest of the earth, if the rotation took place in an hour and twenty-four minutes. This rate of rotation would in fact be much too great for cohesion. Calculation shows that, assuming the earth of the same mass as at present, and taking such a constitution of its interior as seems fairly probable in the time of the earth's fluidity through intense heat, a rotation once in about three hours would have been the most rapid which could have existed without the separation of the equatorial parts of the earth from the rest.

I may note that, although it may be considered by many a cautious mode of procedure to take no account here of the possibility that, at the remote time when the moon was born, the earth may have had a much smaller mass than at present, or again, to consider the possibility that at that time a great portion of the earth's mass may have been vaporous, I do not myself recognise extreme caution in this, but excessive daring, or rather wild recklessness. Considering the multitudes of meteors which fall even now, after many tens of millions of years during which the meteoric supply of the solar system has been undergoing a process of exhaustion, it appears to me we are as much bound to trace back the process of meteoric downfall which we know to be constantly taking place, as to trace back that slow change in the length of the day, and in the distance of the moon, which mathematical analysis shows to be in progress. We cannot escape the conclusion that in past ages the process took place at a much greater rate than at present. We may fairly enough believe that when the earth was in its vaporous condition it was very much larger, though much less massive than now. The conclusion is absolutely inevitable that in each circuit around the sun the earth captured then a much larger number of meteoric masses than at present; for then they were much more richly strewn, and the earth herself was much larger. Even now it is calculated that she captures three or four hundred millions of meteors of all orders in the course of a year. Then, it seems no rash inference, she captured hundreds, or perhaps thousands, where she now captures one. Such a process taking place during fifty or perhaps a hundred millions of years, cannot but have added enormously to the mass of the earth. It is certainly most unsafe to neglect a process which assuredly took place, and as certainly must have had a large share in modifying the conditions under which the processes, considered by Mr. Darwin, took place.

If we take, however, no notice of what nevertheless is certainly a most important point in the problem, we go back to the time when the earth, having something like its present mass, and being in a partial or wholly fluid state, with a volume not differing greatly from its present volume, rotated on its axis in about three hours. All the evidence we have shows us that the earth, at the remote epoch to which we are thus led, must have been very different in all respects from what she is now; the giant planets which are nearer to that earlier stage of planetary existence, *are* very unlike the earth; the sun, which is still younger, so far as development is concerned, differs from her still more; but a judicious scientific caution leads us to disregard evidence of this sort, and to assume what certainly is not the case rather than to make any allowance for changes whose exact amount we are unable to estimate: this is so much the more judicious that the whole problem is one in which the elements are doubtful to a greater or less degree. To speak seriously, no investigation of the problem attacked by Mr. Darwin can be regarded as sufficient, which does not take into account—(1) the probable gaseity of a large portion of the earth's globe at the time when the moon's mass was separated from hers; (2) the consequent small mean density of the earth, or, which is the same thing, her large volume (as compared with her mass); (3) the circumstance that no small portion of the earth's present mass must have been added by meteoric downfall since the time when the moon's mass was separated from hers, the moon having also gained greatly in mass since that remote epoch.[1]

With this proviso, which in no sort affects the general inference deducible from Mr. Darwin's reasoning, though it very largely affects our estimates of the time-intervals corresponding to the changes which we have to contemplate, we resume the study of his conclusions.

At some time, then, very far back in the remote past, and when the earth was rotating much more rapidly than at present, the mass subsequently to form the moon was free to separate itself from the earth, or was already separate, though close to the earth, or was compelled to separate itself; it is not easy to say which view of the three we should adopt. The mass may have been a separate ring, or a single body, or far more probably a ring of small bodies.

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[1] Assigning to the earth, then, a volume—owing to smaller density—eight times her present volume, and therefore a surface four times as great as at present, regarding those meteoric members of the solar system which were capturable (because of the position of their orbits) by the earth as 1,000 times as numerous as at present, and diminishing uniformly in richness of distribution to the present time, setting that epoch 100,000,000 years before the present time, and regarding the average weight of meteors in Prof. Newton's calculations (by which 400 millions of all sorts reach the earth each year) as only 10 oz. (a fair enough allowance when some single ones weigh hundreds of pounds), the total number of meteors which have fallen on the earth would be:

$4 \times \frac{1000}{2} \times 100,000,000 \times 400,000,000 \times 10$ oz.
$= 800,000,000,000,000,000,000$ oz.
$= 50,000,000,000,000,000,000$ lbs.
$= 22,322,000,000,000,000$ tons. As the estimated mass of the earth at present is about 6,000,000,000,000,000,000,000 tons, this may seem but a small aliquot part, yet even as thus estimated it is a great deal too large to be neglected. Considering that Bischoff assigns 350 millions of years to the period during which the earth has cooled from 2000° C. to 200° C., it will be tolerably obvious that we have much underrated the earth's growth.

Dr. Ball is careful to show how the scar left when the moon's mass was separated from the earth's gradually closed up, and eventually disappeared. 'I can easily imagine,' he says, 'an objector to say: "If the moon were merely a fragment torn off, how can we conceive that it should have that beautiful globular form which we see? Ought not the moon to have rugged corners and an irregular shape? and ought not the earth to show a frightful scar at the spot where so large a portion of its mass was rent off?" You must remember,' he proceeds, in reply to this imagined objection, 'that in those times the earth was not the rigid solid mass on which we now stand. The earth was then so hot as to be partially soft, if not actually molten. If, then, a fragment were detached from the earth, that fragment would be a soft yielding mass. Not for long would the fragment retain an irregular form; the mutual attraction of the particles would draw the mass together. By the same gentle ministrations the wound on the earth would soon be healed. In the lapse of time the earth would become as whole as ever, and at last it would not retain even a scar to testify to the mighty catastrophe.'

I believe that the separation of the moon from the earth took place under conditions very different from those here considered: that no irregular mass was torn off from the earth, no ragged gap remained at the place whence the moon's substance had been removed. It seems to me impossible to conceive any process of steady change which could have culminated in the imagined catastrophe. Mr. Darwin considers, indeed—and very likely he is right (though the idea is, as he admits, a mere speculation)—that the tide raised by the sun's influence in the mass of the rotating earth, may have led to the separation of the moon's mass. But this would not have happened, if it happened at all, as a single event. Remembering that the time when this action would be most effective would be the time when no part of the earth's globe was solid—and that stage of the earth's history must have lasted for millions of years—it is obvious that the tidal swing might very well have increased (in the manner suggested by Mr. Darwin) until some portion of the top of the tidal wave was left outside the earth. But the portion thus left would be but small, and immediately after the tidal wave would be reduced in height. (There would probably be two masses thus, as it were, thrown off, one from each of the two opposite tidal waves.) Gradually after this the wave would increase again in height, and again the undue elevation would result in the throwing off of matter from the top of the great tidal wave. The process would be repeated again and again, each mass thus thrown off slowly retreating, owing to the action of forces similar to those which cause the present slow retreat of the moon. The bodies thus thrown off would form a flat ring in the plane of the earth's equator—a ring probably similar to that which now surrounds the planet Saturn. It is to all intents and purposes certain that the mass which was eventually to form the moon's mass was thrown off in this gradual manner. When we see in the Saturnian system a ring precisely like that which must thus have been found around the earth, we are justified in finding 'confirmation strong' of the theory as to the moon's formation which Mr. Darwin has advanced.[1]

Mr. Darwin himself, indeed, considers that the moon's mass was thrown off at a single effort as it were. The reasoning relating to this part of his views does not belong, indeed, like the rest, to the sure domain of mathematics, but to speculation. Let us, however, follow it as presented by an astronomer who apparently accepts the reasoning as sound—Dr. Ball :—'One hint,' he says, 'dynamics does give. It reminds us that a rotation once in three hours is very close to the quickest rotation which the earth could have without falling to pieces. As the earth was thus predisposed to rupture, it is of extreme interest to observe that a cause tending to precipitate such a rupture was then ready to hand. It seems not unlikely that we are indebted to the sun as the occasion by which the moon was fractured off from the earth and assumed the dignity of an independent body. It must be remembered that the sun produces tides in the earth as well as the moon' (that is, as the moon does). 'The solar tides are small compared with the lunar tides . . . but before the moon was detached the earth was disturbed by the solar tides alone. The primæval earth thus rose and fell under the tidal action of the sun. Probably there were no oceans then on the earth ; but tides do not require oceans or even water for their operation. The primitive tides were manifested as throbs in the actual body of the earth itself, which was then in a more or less fluid condition. Even at this moment bodily tides are disturbing the solid earth beneath our feet ; but these tides are now so small as to be imperceptible when compared with the oceanic tides. . . . Suppose now that the liquid primæval globe were pressed in on two quadrants and drawn out on the two others, and that the pressures were then released. The globe would attempt to regain its original form ; but this it could not do at once, any more than a pendulum can at once regain its vertical position' (after being swung) ; 'the protruded portions would go in, but they would overshoot the mark, and the globe would thus oscillate to and fro. Now, it has been shown that the period of such oscillations in our primitive globe is about an hour and a half, or very close to half the supposed length of the day at that time. The solar tides, however, also have a period half the length of the day. Here, then, we have a succession of small impulses given, which are timed to harmonise with the natural vibrations. The solar tides raised threw the earth into large vibrations. At first these were small, but at each succeeding impulse the amplitude was augmented until at length the cohesion of the molten matter could no longer resist : a separation took place: one portion consolidated to form our present earth ; the other portion consolidated to form the moon.'

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[1] I may note here that in the preface to *Saturn and its System* (the first book of my writing) there occurs the following passage, in which I made a prediction, very strikingly confirmed, I think, by the relations above indicated: 'It is not impossible that in the variations perceptibly proceeding in the Saturnian ring-system a key may one day be found to the law of development under which the solar system has reached its present condition.'

It is not safe to assert what would or would not happen under conditions utterly unlike those with which we can deal in actual experiment. But so far as I can judge from all the known properties of matter, I am led to believe that no such wave as is here considered by Dr. Ball, and as Mr. Darwin had already indicated as likely to arise, could by any possibility come into existence. Long before it had attained anything like such dimensions as this theory of moon generation requires, cohesion between its parts must have ceased. Consider the mere increase in the centrifugal force in the crest of such a wave, and it will become apparent that when the earth was rotating at the critical rate which barely kept its equatorial parts together, a much smaller vibrational rise would result in separation. It seems to me as absolutely certain as anything not falling within the domain of actual experiment and observation can possibly be, that the great tidal vibration would break into spray long before it reached such a height that the portion free to separate was comparable in mass with our own moon.

We may note, too, that, while the appearance of the Saturnian rings corresponds with the manner of moon generation here considered, which in itself is a strong argument in favour of the theory, we may view the Saturnian rings in another way which even more strongly suggests that this is the actual way in which moons are born :—

We see in the Jovian system four moons, all fully formed, the innermost still very near to its parent orb; and we find Jupiter himself in a condition, judging by his mean density, corresponding to what we may suppose to have been the earth's condition when the moon, after being fully fashioned, had receded to a corresponding distance (that is, to something like the same relative distance from the earth). Mr. George Darwin has, indeed, shown in a very interesting discussion of Jupiter's compression and the motions of his moons, that from something more than the mean density of the planet this condition of Jupiter may be inferred. It is as nearly demonstrated as such a relation can well be, that the central part of Jupiter is greatly compressed, compared with the outer parts of the planet as we see him. Now Saturn, judged by his mean density, appears to be in an earlier stage of his career as a planet than Jupiter. While Jupiter's mean density is but a fourth of the earth's, Saturn's is barely one-seventh of hers. We may somewhat safely infer, then, that Saturn's moon-generating work is not so far advanced as Jupiter's; and, in fact, considering his moons only, we see that this really is so, for his nearer moons are much closer to him, absolutely as well as relatively, than are those of Jupiter. It is true Japetus, the outermost Saturnian moon, has receded to a greater absolute distance than the outermost moon of Jupiter, and Saturn being a smaller planet than Jupiter, this greater absolute distance of Japetus implies a relative distance greater in still higher degree. But this only serves to show that, from whatever cause, the moon-generating process in the case of the Saturnian system has progressed more slowly than in the case of the Jovian system. It is at any rate clear that the innermost of Saturn's moons is relatively much younger than the innermost of Jupiter's. This being so, what opinion are we to form of the ring-system? Does it not, on the face of matters, appear as though this ring-system represented an embryo moon, or perhaps the embryos of several moons? Finding thus around the planet of least density (presumably, therefore, the one which has advanced least towards its final condition), the planet which has the nearest moons, and, in fine, the planet which—if such planet there is—must be regarded as alone in the moon-generating stage of planetary existence, this singular appendage, absolutely unique in the solar system, are we not justified in saying that here we see the last stages of the moon-producing stage of a planet's life? It seems to me that this is the most probable interpretation of the rings—if it be not the only interpretation available. If we accept it, we see what a moon is like when as yet not fully fashioned. It consists not of a single globe, not of several large globes one day to condense into one, but of rings of multitudinous bodies, strewn so closely that, from a distant observing-station, they appear to form continuous solid or liquid rings. If this were an interpretation of the Saturnian rings to which the discussion of our own moon had led us, a certain degree of hesitation might be suggested by the circumstance that possibly our interpretation so deduced might be a little forced. But the reverse of this holds—we are encouraged to adopt the view instead of being led to doubt it—when we note that more than seventeen years since, the Saturnian rings were proved to be constituted in the manner here described. Nothing can be much more complete than the demonstration of this which was given by the Bonds and Prof. B. Peirce in America, and Prof. Clerk Maxwell in England. There cannot now be a shadow of doubt that the entire Saturnian ring-system consists of discrete satellites, as the sands of the sea-shore for multitude, richly aggregated in some parts of the system's breadth, sparsely strewn in others. Now, this being presumably an embryonic moon-system—perhaps to form one moon, perhaps to form several—we have strong evidence in favour of the belief that a moon is thrown off from the parent planet in this form, and not as a single body; for certainly there is no reason for supposing that the process of moon-formation in Saturn's case would be different from the corresponding process in the case of any other planet.

We may draw yet another inference from the giant planets as to the past of the earth and moon. So far as we can judge from Jupiter and Saturn, a planet remains in a partly vaporous, partly fluid state long after the moons have been formed, and have receded to a great distance from the parent orb. We may perhaps assume, indeed, that it is while a planet is in such a state that the forces thrusting a moon away from its parent planet's neighbourhood are most active. Unquestionably, when a large part of a planet is fluid, so that a great wave of fluid or plastic matter circuits around the planet, while what is one day to become the planet's ocean exists only in the form of steam or cloud or falling rain-showers, the forces at work checking the rotation of the planet, and *pari passu* repelling the infant moon, would be far more active than when the chief retarding agent was an oceanic tidal wave. Combining with this the consideration that when the moon was nearer its tide-raising power was greater—not as the inverse square, but as the inverse cube of the distance—we must attribute to the earlier stages of a moon's independent career the greater part of its work in checking the rotation of its parent planet, and thus (indirectly) causing its own repulsion from that body's neighbourhood.

Thus I cannot for my own part consider that much of the work done by the tidal wave in forming the earth's crust was effected, as Dr. Ball believes, when the moon was much nearer to the earth than now. That within the range of time over which the geologic record extends the moon's action was much more effective than it is at present, we may well believe; but that, at any time while the earth's fossiliferous strata were being formed, the moon was within 40,000 or even 100,000 miles from the earth I cannot regard as likely, or even credible. If the long time-intervals necessary to explain the features of the earth's crust could be greatly shortened by such considerations as Dr. Ball has eloquently urged, the case would, perhaps, be different. There is an enormous difficulty, unquestionably, in reconciling the vast period (100 millions of years at least) during which the earth seems to have been acted upon by the solar rays as at present, with the comparatively short period (not more than twenty millions of years) during which the sun can have done such work as at present, if his emission of heat is regarded as solely due to his contraction to his present dimensions. But we cannot evade the difficulty by appealing to the moon's former tide-raising energies. There are other lines of argument besides Dr. Croll's by which the vastness of the period during which the sun has worked as he does now in the emission of heat and light can be demonstrated. Either our interpretation of the source of his heat is incorrect (or at least incomplete), or else, as for my own part I believe, the process of solar contraction has gone much farther than those infer who imagine that the sun's real globe is nearly of the dimensions of that orb which is bounded by his photosphere or light-surface. But be this as it may, there can be very little doubt that when the moon was but 40,000 miles from the earth's surface, there was no life on the earth, no surface which could support life. In all probability she was in the same state as Jupiter—her surface so hot that the waters which were one day to form her oceans were kept constantly by intensity of heat in the form of vapour, save where, at a great height from the fiery surface below, they were condensed to the form of visible clouds.

As regards the future of the moon, in which is involved to some degree the future of the earth, we may accept the general conclusions of Mr. Darwin and Dr. Ball, though the estimate of the time-intervals which must elapse, ere the successive changes are reached, cannot be regarded as trustworthy. (The problems involved are far too complex to be satisfactorily dealt with in the present stage of the discussion: I doubt even whether science will have ascertained, a thousand years hence, the true rate at which the moon's recession will take place during the next ten millions of years.)

In the first place it is to be noted that the terrestrial day is now shortening more quickly (we ought rather perhaps to say less slowly) than the lunar month is lengthening—so that, though the month is lengthening, the number of days it contains is gradually diminishing. It was otherwise in the past. The number of days in the lunar month continually increased until the time when the month lasted about twenty-nine days, since which time the number of days in the month has continually diminished. Dr. Ball describes the time when the number of days in a lunar month was at its maximum as the time when the month was in the zenith of its glory,—*why*, this deponent sayeth not, not knowing. Measured in any other way than by terrestrial days, the month grows constantly longer, and will do so until the moon no longer has any work to do in retarding the earth's rotation. This is the same as saying that the lunar month will continue to lengthen as long as it differs from the terrestrial day. Thus, great as the period would be during which the day would have to lengthen to equal the present lunar month, we have to look forward to a still greater distance in the remote future for the time when the lengthened day, and the less lengthened lunar month, will be equal. At that time the day and the month will each last 1,400 hours, as hours are now measured, or 58⅓ of our present days. Dr. Ball puts the time when this change will have been effected 150,000,000 years from the present time. It appears to me his estimate falls far short of the truth. The actual lengthening of the day, noted since the time of Hipparchus, has accrued at a much slower rate than Dr. Ball's estimate would imply. However, even if the epoch be no more remote than this, we need not fear that the progress of the change will seriously affect either ourselves or our descendants for many generations to come. Probably long before ten millions of years have elapsed, much more important changes will have affected the earth—as loss of solar heat, the effect of long-continued internal changes, such as are now in progress, and so forth. We may be as easy respecting the lengthening of the terrestrial day, on account of the great remoteness of the final condition, as we may be respecting catastrophes threatened as nearer at hand—on account of their improbability.[1]

Admitting the possibility that, at the remote epoch when the change has been effected, there may be reasoning beings upon this earth, we may accept the fanciful ideas suggested by Dr. Ball. 'Our remote posterity,' he says, 'will have a night 700 hours long, and when the sun rises in the morning, 700 hours more will elapse before he can set. This,' he adds (though we should suppose he can hardly be very confident on this point), 'they will find a most suitable and agreeable arrangement. They will look back on our short periods of rest, and short periods of work, with mingled curiosity and pity. Perhaps they will even have exhibitions of eccentric individuals able to sleep for eight hours, work for eight hours, and play for eight hours. They will look on such curiosities in the same way as we look on the man who undertakes to walk a thousand miles in a thousand hours.' ('All which propositions,' as Carlyle words it, 'I, for the present, content myself with modestly but peremptorily and irrevocably denying.')

But although, immediately after telling us these things, the Astronomer Royal for Ireland adds, 'I am beyond all things anxious to give you the impression that I am not indulging in any mere romance,' we may indeed place a great deal more reliance on what he says later respecting the evidence given by the moon's present rate of rotation. It is utterly incredible that the moon, when first formed, no matter what theory of her formation we accept, rotated anything like so slowly as she does at present. It is to all intents and purposes certain that then—whenever 'then' was—she rotated in much less than 24 hours. Now she requires 27⅓ days for each rotation. There is here evidence of an enormous amount of work done by the earth in raising and maintaining lunar tides, for by such work alone could the moon's rotation rate have been changed to what it now is.

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[1] It has been stated in the *Spectator* that I believe in the probability that all life will be destroyed from off the face of the earth a few years hence. This was at least news to myself. I have discussed the probability that a certain comet will be absorbed by the sun, mentioning some one else's suggestion that such destruction might be effected a few years from the present time; but I have also been careful to explain that what has already happened in the case of this very comet, shows how very small is the chance that the final absorption of the comet will in any way affect the earth's inhabitants. I have scarcely ever mentioned such fears except to ridicule them.

Whether the moon formerly had oceans, as most astronomers believe, or not, matters little. We see from her present aspect that she was once intensely hot, insomuch that the greater part of her substance, if not fluid, must have been viscous and plastic. In that plastic mass the earth raised tidal vibrations, swaying the moon's rotation rate into accordance with her period of revolution round the earth. In the constancy with which the unjustly called 'inconstant moon' turns ever the same face towards the earth, we recognise the long-continued action of these tidal vibrations. As Dr. Ball well says—'Those tides have ceased for ages; their work is done; but they have raised a monument in the moon to testify to the tidal sufferings which the moon has undergone.'

What the earth has done, effectively though slowly, to the moon, the moon will do as effectively, though even more slowly, to the earth. It is this cause of change, of the efficiency of which the moon's calm face is ever speaking to us, that will produce the lengthening of the day, and of the lunar month, which we have already considered.

I do not altogether agree with Dr. Ball as to the future of the earth and moon lying beyond the sufficiently distant future to which we have already carried our thoughts. He points out that besides the lunar there is a solar tide, and that after the former has done its work in bringing the earth's rotation period to coincidence with the lunar month, the latter still checking the earth's rotation, will cause the terrestrial day to exceed in length the lunar month. He considers that in the case of Mars's internal satellite such a change has already been brought about, the satellite revolving around Mars in a period shorter than that of the planet's rotation. It appears to me that the two cases are not analogous. The mystery of the inner satellite, Dr. Ball tells us, 'has never been explained : it is due to the action of the solar tides on Mars ; nay, more, we can actually foresee that at some incredibly remote future time our earth and moon are destined to present the same movements which have seemed so anomalous in Mars.' He appears to overlook the effects which the outer satellite would tend to produce, and also what we notice in the case of our own moon. We can readily understand how, with an outer moon travelling in longer period, the Martian day would have increased in length beyond the time of the inner moon's rotation : whereas we see in our own moon clear evidence that the solar tide has not the power which Dr. Ball here assigns to it—or rather, that whatever effects it may exert in that way, are overborne by greater forces working in an opposite direction. Ever since the moon's rotation-period was brought into agreement (by her earth-raised tides) with her period of revolution, she has been subject to the sun's influence in still further lengthening her period of rotation—this influence being somewhat stronger on her than on Mars, despite her smaller globe. Yet during the millions of years that this force has been at work, it has not in the slightest degree availed to lengthen the rotation period beyond the period of revolution. These periods were, and remain, absolutely coincident. The reason is obvious : the earth has exerted a greater force to prevent such an increase of the moon's period of rotation than the sun has exerted to produce it. In like manner, we may safely conclude that, whenever the moon has wrought the terrestrial day into coincidence with the lunar month, she will continue thenceforth to maintain that coincidence—overruling all the efforts which the sun will make to still further lengthen the terrestrial day.

For my own part, however, I believe that long before that time arrives, every particle of water will have disappeared from the earth's surface—the seas and oceans being withdrawn into the earth's interior as her mass parts with its heat. That any living creatures will exist on the earth at the remote time to which our thoughts have been carried, seems to me altogether improbable.

BIRTH AND DEATH OF WORLDS.

To the child in its nurse's arms the room in which his life is passed seems the whole world. But as the child grows older he finds the room to be but part of a house, the house part of a street, the street part of a city, and so forth. Gradually he gets larger and larger ideas of the earth which is his home. What is true of each individual child, is true of the childhood of the human race. In long-past ages men judged the narrow tract, island, or valley where their lives were passed as the whole world. For that region the heavens were made; the sun rejoiced as a giant to run his course, that he might illuminate that abode by day; the moon was made to be its light by night; sun, moon, and stars to be for signs and for seasons, and for days and years. But gradually men widened their conceptions of the world. They found the home of their tribe to be but part of the region which their race inhabited, this to be but part of a larger region. Wider and wider became their knowledge of the earth, until they found that what they had regarded as the whole world was the merest point on the great globe which is the home of the human race.

But this was little. It may be said that this was nothing. Men had found the earth, their home, to be much vaster than they had imagined; but they presently found that in another sense it is exceedingly minute. They had judged it to be the great fixed centre of the universe, even after they had found that it is not a great level tract of land and water, but a globe. They now found that it is not fixed, but in motion; not the centre of the universe, but a member only of a family of globes, travelling like it around the sun, and very much less than the larger of these, while compared with the sun it is but as a point.

Even this, however, was far from being all; it was indeed but as a short advance towards the grand conceptions of the universe which belong to the science of our day. Just as the earth had come to be regarded as but a point, almost simultaneously with the discovery that it is a vast globe, with a surface of two hundred millions of square miles, so the very discovery which assigned to the sun his real position in the solar system, showed also that in one sense he is but a minute part of the universe. It was rightly objected by Tycho Brahe to the Copernican theory (though the objection was overruled as observation advanced), that if the theory were true, the stars ought to change in apparent position as the earth circled on her wide orbit round the sun. The objection is sound, the stars must change in their apparent position, though in Tycho Brahe's time not one star could be shown thus to move, and even now there are not ten stars whose motions of this sort have been effectively measured. But the Copernican theory is sound all the same. It is the vast distance of each star that causes it to appear all the year round in the same position on the star sphere, though the point from which we observe the star circles year by year round the sun in a mighty orbit, a hundred and eighty millions of miles in diameter. Even in the time of Tycho Brahe, when the sun's distance was supposed to be but a few millions of miles, it was seen that the only possible explanation of the stars' apparent fixity of position involved such vast conceptions of their distances as to compel us to believe that they are suns scarcely less important than our own.

But as astronomers increased their estimate of the sun's distance, and as, observing more and more carefully the stars' positions, they diminished the possible range of the as yet undetected apparent motions, men's conceptions of the grandeur of the material universe increased. With Briarean arms science thrust back the stars into the depths of space, until the glories of the nocturnal heavens were changed from so many thousand points of light to as many suns, many as grand as our own, many far grander, some, like Sirius, Vega, and Canopus, so much vaster than he is, that by comparison with them he seems the merest miniature of a sun.

But even this, stupendous though it seems, is little compared with the scene presented when we rightly interpret what the telescope reveals respecting the depths of space beyond the domain of the visible stars. For each star we can see, thousands were made visible by the telescope of Galileo, in later times tens of thousands, and in the days of the elder Herschel hundreds of thousands. With the best telescopes in our own time it is probable that as many as a thousand million stars could be seen were every part of the celestial sphere examined.[1] A thousand million suns, a thousand million repetitions of the glories and the wonders which modern science reveals in the central orb of our system!

But the very progress of our knowledge of the vastness of God's domain in space has taught us to recognise the incompleteness of our knowledge. When men supposed that the orbs they see in the heavens represent the whole glory of the universe, they were content with what they saw, and supposed they knew all. Now that we know how little men knew in past ages, we no longer believe that we can now extend our range of view over more than the manifest portion of the real universe.

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[1] This estimate is greater than that usually adopted. Chacornac considers that with the gauging telescopes of the Herschels 20,000,000 stars could be seen. But if we consider that the 2½-inch telescope used by Argelander in his Durchmusterung showed no fewer than 324,000 stars in the northern heavens, clearly enough for their places to be exactly determined, and would certainly show 500,000 in each hemisphere clearly enough for counting, it is evident that Chacornac's estimate of the number which could be seen with the 18-inch mirror of the gauging telescope is far too low. Assuredly, if a 2½-inch telescope shows more than a hundred times as many stars as the naked eye, it is certain that the great gauging telescopes would show more than a hundred times as many as Argelander's, or more than a hundred millions, and as certainly there are many telescopes in the present day which will show more than ten times as many as the Herschelian telescopes.

It would have been natural, but strangely enough it did not happen, that men's ideas of the duration of time should have grown, *pari passu*, with their ideas of the vastness of space. So long as they had recognised as the entire universe a small rounded region of the earth's surface, arched over by the dome of the sky, in which certain bright objects, sun and moon and stars, seemed to travel, a very moderate time-interval seemed long enough for the entire duration of the universe as well in the future as in the past. But with every increase of men's estimate of the extent of the universe came an increase in their estimate of the duration of the time during which the natural universe had existed in the past and would continue to exist in the future. Only, in one case, proof positive forced men to enlarge their conceptions; in the other, abstract reasoning alone was at first available, and even later the proof was not so absolutely convincing (at least to the weaker order of minds) as that which showed the vast extension of the universe in space.

Applying this principle, let us see what we must think of time past and to come. Our views of the vastness of space have ranged from the comparatively minute to the inconceivably vast, and beyond that we look into the depths of the immeasurable, the infinite. So it must be with time. From the few thousands of years which seemed to suffice for the small universe of the ancients, our thoughts have risen to the recognition of millions on millions, nay, of millions of millions of years, and they should pass beyond these, if we rightly apprehended the teachings of science, to the contemplation of absolute infinity of time in the past as in the future.

But as our ideas as well of the vastness of space and the duration of time have grown, so also should our estimate grow of the range of the operation of what we call law. In past days men might perhaps reasonably fear that if they extended their belief in development beyond what they could unmistakably recognise in the growth of animal or plant, they would see evolution extending its domain throughout the entire universe, as they knew it. But as their estimate of the universe extended, this fond fear vanished. From a plant to a forest, from a forest to a flora, they felt they could safely carry their survey of the vegetable world. From an animal to a species, from a species to a fauna, they could safely trace the evolution of animals. Even more widely might their sway be extended. Until at length men saw that they might recognise evolution throughout the entire domain of the universe as they knew it, and yet be no nearer the First Cause, than of old they were in believing that the tiniest plant or animal grows according to natural laws.

It is in this spirit, in the full belief that so far as man can extend his survey he can trace the existence of law, without even coming near the First Cause (for what relation save that of absolute nothingness can the finite bear to the infinite?), that we approach the consideration of the development of a small part of the universe—the merest atom in space—our solar system, the sun, with his family of dependent worlds.

Science considers it now as to all intents and purposes established that this universe

> in tracts of fluent heat began
> Till toward the centres set the starry tides
> That eddied into suns, that wheeling cast
> The planets—then the monster—then the man.

It may be doubtful in what precise manner the process took place, but that each orb in our solar system, for instance, was once a mass of vaporous matter widely extended in space, no modern student of astronomy doubts.

The first stage, then, in which we recognise a world or planet like our own is that of a mass of glowing vapour. And when I say vapour, I do not mean one kind of vapour only, but that every substance which now exists in this world in the solid or liquid state was once, through intensity of heat, in the form of vapour.

Now let us consider what are the characteristics which we might expect to recognise in a world in such a condition as this. First, it is clear that, as only a very intense heat could convert into the vaporous form the substances which we know on the actual earth as solids, an orb in this primary state of a world's existence would be so hot that we might fairly expect its heat to be felt even at such distances as separate planet from planet, or the planets from the sun. Nor can we doubt that when there was such intense heat there could be also most brilliant light. Then again, the vaporisation of such a world as ours would necessarily be accompanied by a vast expansion of its size, and therefore by a great diminution of its mean density, so that we cannot doubt that an orb in the first or vaporous state of its existence would have a smaller mean density than later, when its materials had become in great part solid or liquid.

Such, then, would be the characteristics of the first stage of a world's life, and by such characteristics may we know a world in this stage of its career, if any such now exist: intense heat, heat that can be felt and measured, brilliant lustre, and small mean density. The reader will see presently why we note these features.

And now let us consider the next stage through which such a world as we have considered would have to pass.

After a time the radiation of heat from the mass of glowing vaporous matter would result in so great a diminution of the heat originally pervading the mass, that a large portion of the matter which had been vaporous would become first liquid, then solid. The substances, for instance, which now form the solid crust of the earth would thus become solid, and so would a large proportion of the material now existing in that form. Others of these, though not becoming solid, would be changed to the liquid form. But there is one substance existing in large quantities now in the liquid form, which assuredly could not become liquid for a very long time after the earth first began to have a partly liquid and partly solid nucleus. We refer to water, which covers now three-quarters of the earth's vast surface of 200 millions of square miles. When the earth's first formed surface was still glowing with its primeval fires, it is manifest that water could no more have rested on that surface than it can now rest on a surface of redhot iron. We do not say that every part of it would of necessity be converted into steam or vapour. We do not even say that no water would be in contact with the fiery hot surface. We say simply that it could not rest against that surface. Where the pressure is so enormous as at the bottom of our present seas (for instance), water may remain in contact with a surface many times hotter than boiling water and yet not be converted wholly into steam. Yet it could not possibly rest under those conditions. Vast quantities of steam would rush upwards through the superincumbent water (which would be partly vaporised by the intense heat of the uprising vapour) passing with enormous turmoil and uproar into the higher air. And unquestionably, if the whole surface of the earth were at such a heat as that of glowing iron, the greater part of the waters now forming our seas and oceans would be always in the form of vapour, while the portion which would remain liquid would be constantly interchanging that condition for the vaporous one, the supply remaining tolerably constant, because, as fast as water was turned into steam and flung upwards, vast quantities of water formed by the condensation of the immense clouds constantly brooding over the fiery earth would rush down in torrential showers to take its place.

Now let us consider what would be the aspect of a world in this second stage of orb-life, which we may call the fiery stage.

In the first place, it is tolerably clear that, seen from without, such a world would present only the outside of its cloud envelope to view. In proportion as the quantity of steam forming a part of the complex atmosphere of the earth was enormous, so also would the cloud masses enwrapping the earth be vast and the layers formed by them be deep, and impenetrable either from without or from within. Above our earth, even in its present condition, three distinct layers of cloud frequently form over one and the same region, besides other but less distinct intermediate layers. There are on the outside the light feathery clouds called cirrus, really consisting of minute particles of ice. Next to these, but far below them—usually perhaps at least nine or ten miles below—are the woolpack clouds called cumulus, showing where great quantities of aqueous vapour have been changed into the form of visible cloud. Below these again, and often clinging to the very surface of the earth, are the clouds in which actual precipitation of rain is taking place, the so-called nimbus clouds. Between these layers may be often seen the cirro stratus and the cumulo stratus. Now if this is the case with the present clouds, formed under far more stable conditions and from a comparatively insignificant quantity of the vapour of water, how many and how complex must have been the cloud layers, how deep and dense must they severally have been, when millions of millions of tons of water were present in the atmosphere in the form of steam !

We might certainly expect, then, to find a world in that second or fiery stage of world life, presenting a surface manifestly formed in great part, if not wholly, of cloud masses. We might also certainly expect to find the surface so seen undergoing frequent, and sometimes very rapid, nay, tremendous changes. Thirdly, since the apparent size of such a globe would be that corresponding to the outermost surface of its outermost cloud layer, it would appear much larger than it really was, and would be judged by observers unaware of its real condition to be of much smaller mean density than was actually the case.

Such would be the characteristics of an orb in the second or fiery stage of a world's life: a cloud surface, indications of intense activity, and relatively small mean density.

Then would come the third or middle stage of planetary life, for which the two preceding stages had been merely preparatory. The surface would become at last so cool that the waters which had hitherto been suspended in mid air, in the form either of steam or of cloud masses, would be able to rest on the surface as seas and oceans. Dry land would appear, life would become possible on the transformed world. Whether, under the conditions existing just at this stage of the world's existence, life could in any way come spontaneously into existence, may be a subject for speculation or perhaps hereafter even for theorising. Science, at any rate, knows nothing now of the possibility of spontaneous generation, so that the peopling of a world with life is as great a mystery now as it has ever been. Men of science may amuse themselves by speaking of life being brought to the earth by the arrival of a meteor, in reality a fragment of some once peopled world, which has been destroyed by conflict with another or by internal disturbance. But this is more a scientific jest than a grave reality. Astronomy knows nothing of worlds coming into conflict. On the contrary, the laws of motion assure us that if anything is so unlikely that it may be regarded as absolutely impossible, it is the encounter of two orbs in mid space; nor have we any reason to suppose that a planet can be rent into fragments by internal convulsions. If we had, we have not the slightest reason for supposing that orbs thus unfortunate would be more likely to be inhabited than their more lucky fellow-worlds. If these were inhabited already, we gain nothing by bringing to them the fragments of other worlds which have exploded; and if they were not inhabited, while the burst or shattered worlds were, we are called on to imagine (for no one can believe) the absurdity that only inhabited worlds are liable to destruction, for the benefit of those which are without inhabitants. To which absurdity this additional one is superadded, that the seeds of life would survive the destruction of their planet home, and the journeying through millions on millions of years (rather millions of millions) which science assures us they would have to make through the cold of interstellar space, before they would fall on any other world. And all these absurdities to no purpose, so far as the origin of life is concerned, for they take us back but a step, which brings us in reality no nearer to the beginning of all life.

But we are not concerned to inquire here how life was introduced upon this earth or any other world. We know that in some way life began after the earth became fit to be the abode of life; and again we need not inquire what would be the appearance of a planet in the life-bearing stage, seen from a distance, as we see Mars or Venus. For we require no extraneous evidence to show that an orb passes through the life-bearing stage, seeing that we know from our own earth that this is the case.

We may pass on, then, at once to consider the later stages of planetary existence.

Here we no longer have the assistance which we have in the case of past stages, from a study of the earth's actual condition. It is easier to infer from the earth's present state what her past has been, than to predict what her future will be. For the traces of past changes remain, the effects of such changes having as it were accumulated, but the signs of changes yet to take place are not so obvious, because the stages of a world's life last very long, and the amount of change which can be actually observed in the lifetime of a man, or even of a race, is almost imperceptible.

But there is one change which, though in such periods as we have just named it hardly manifests any effect at all, must yet be all the time in progress. It was shown long since by Sir Isaac Newton (and though the method of his reasoning is rather out of date, the principles on which it rested are sound enough) that the seas and oceans of this earth must all the time be diminishing, though so slowly that in many generations no visible change of level can be perceived.

The theory which with Newton was little more than a speculation has in our own time been placed on a secure footing. Advanced almost simultaneously, but independently, by Saemann in Germany, by Stanislas Meunier in France, by Dr. Sterry Hunt in America, and by Frankland in this country, it may now be regarded as established on a basis of strong probability, to say the least.

In the long periods of time which belong to the earth's lifetime, processes which in the lifetime of a man or in the history of a nation produce scarcely any discernible effect may so act as entirely to modify the aspect of a world. It becomes more and more clear, as we study the earth's history, that it must be measured, not by thousands of years or by tens of thousands, but at the very least by millions of years. Now, let us imagine that the rate at which the water in our seas and oceans is withdrawn into the interior is so slow that in a single year the sea-level is reduced by an amount equal to about the thickness of a sheet of paper. Then in a hundred years the depth of the sea would be diminished only a single inch. At this rate, in about 6,400,000 years the sea-level would be reduced a full mile, and in 60 millions of years every trace of water would have disappeared from the surface of the earth.

Here we must not fall into the mistake of describing the water as withdrawn into cavities formed within the earth as she cools. It does not seem to be so generally recognised as it should be that below a certain depth there can no more be cavities within a planet's globe than there can be holes in the masses of water forming the seas and oceans of the planet. The pressures which exist deep down below the surface, at a depth for instance of 20 miles, are so great that the hardest substances at such pressures become plastic and behave like fluids. In Tresca's experiments, for instance, on the behaviour of steel under great pressure, it was found that the metal behaved like a viscous fluid; yet the pressures Tresca could apply were as nothing compared with those which exist at a depth of twenty miles below the surface of the earth. Thus, at such depths there could not be cavities of measurable dimensions. Such cavities, however, as exist in porous bodies, could and would exist in the crust and throughout the frame of a cooled planet, and into such cavities the waters of the seas and oceans would unquestionably be withdrawn by the action of capillary attraction.

Within periods of time such as correspond to the lifetime of a planet, the water on our earth will be perceptibly diminished and eventually will disappear altogether.

In an old planet, then—that is, a planet which has passed from beyond the life-bearing stage in which our earth now is—the water surface is much less than in the time of mid-life. We should recognise a planet in this its period of decrepitude, by the smaller extent of its water surface. If we supposed all planets to be fashioned on the same general model as our earth, we can by studying deep-sea soundings ascertain even the general shape and appearance of the diminished seas which would then remain. We find, when we have done this, that the smaller seas would have such shapes as we recognise in the Baltic—there would be many seas lying, so to speak, within continents, but connected by comparatively narrow inlets with the oceans, which would still remain, though much diminished in extent. If there is a world within the range of telescopic vision which is in this stage—the fourth—having passed through the glowing vaporous stage, the fiery stage, and the life-bearing stage, we might expect to recognise it thus by its diminished sea-surface, and by the shape of its smaller seas.

The last stage—when no trace of life could remain—would be attained when every trace of water had disappeared from the planet's surface. We should recognise a planet in this stage by the utter absence from its disc of every trace of cloud. It is probable that with the disappearance of all the water the atmosphere of a planet would be greatly reduced in density and extent. But even if the air remained, it would be a perfectly dry atmosphere. No clouds could form, no rains fall, no changes such as we regard as meteorological could possibly take place. The planet's surface would present a dead, arid waste.

Such, then, are the five stages of a world's life—the glowing vaporous stage, or infancy, the stage of fiery youth, the life-bearing middle stage, the time of decrepitude, and finally the stage of death.

But if every orb passes through these stages, we might fairly expect to find examples of every one of them, or at any rate of others besides that stage of middle life in which our own earth is, among the numerous orbs which the telescope enables us to study. For we can hardly imagine (on *à priori* considerations) that all the orbs in each system would be in the same stage of a world's existence. Even if we supposed, which we have not the slightest grounds for doing, that they had all started into existence at one and the same epoch, we should yet expect to find that the stages of life in bodies which differ so much in size would differ considerably in length. Were this so, if they all began their career as worlds at the same time, they would attain each stage of their life at very different times. This moment, for instance, which belongs to the mid-life of the earth, might correspond to the fiery youth or even to the vaporous infancy of another world, and to the old age or even the final or deathlike stage of another.

But before looking around among the orbs of space, may we not adopt some law for our guidance by which we may form some idea as to which worlds are likely to be young, and which to be old?

If we consider that the stages of a planet's life depend on the degree of temperature to which the substance of the planet is raised, we shall see that such a law as we have mentioned may be deduced from a consideration of the laws according to which heated bodies cool. The planets, we may assume, are made of the same materials, though of course such materials may be differently proportioned in different planets. Thus we need not consider the different rates at which bodies of different materials cool. We know, however, that the planets differ greatly in size. Now, bodies of the same material, but of different size, take unequal times in cooling. If we heat two globes of iron, one an inch in diameter, the other two inches in diameter, to a red heat, and set them to cool, we find that the larger takes a much longer time in cooling than the smaller. When the former has lost most of its former ruddy lustre, the larger still remains aglow with heat. When the smaller has at length become so cool that it can be readily handled, it would be unpleasant to try the same experiment with the larger. When we ask why this is, we readily find an answer. The amount of heat that a body contains is proportional to the mass of the body. In the case of two globes of iron, for instance, at the same temperature, one globe being one inch, the other two inches in diameter, the quantity of heat which the larger globe has to part with exceeds that possessed by the smaller globe, in the same degree that the larger globe exceeds the smaller in mass—that is, as eight (the cube of two) exceeds one. But the heat can only pass away at the surface, and the surface of the larger globe is not eight times, only four times, as large as that of the smaller. Since, then, when both are at the same temperature as at first, the larger has eight times as much heat to part with as the smaller, and since it parts with that heat only four times as fast as the smaller (or at only half the rate at which it would have to part with its heat to cool as fast), it follows that the heat of the larger will last twice as long as that of the smaller. Or, to speak more exactly, each stage of the cooling of the larger lasts twice as long as the corresponding stage in the cooling of the smaller. This is exactly the degree in which the diameter of the larger exceeds that of the smaller. And a little consideration will show that in this degree always (that is, in the proportion of the diameters) the cooling of the larger of two globes will last longer than that of the smaller.[1] For instance, suppose the larger three inches and the smaller two inches in diameter, then the mass of the larger exceeds that of the smaller twenty-seven times, while the surface of the larger exceeds that of the smaller only nine times. Since, then a supply of heat twenty-seven times as great is parted with, not twenty-seven times, but only nine times as fast, it follows that the duration of the supply will be greater in the same degree that twenty-seven exceeds nine, or three times, and this is the degree in which the diameter of the larger globe exceeds that of the smaller.

Now, it would be by no means safe to assume that the duration of each stage of a planet's life will be proportional to the planet's present diameter. For the planets do not resemble the globes of our reasoning in being of the same mean density. We might be nearer the mark, perhaps, in supposing that the duration of a planet's life is proportional to the cube root of the number representing the planet's mass, for then we should be taking the diameters which the planets would have if they were all at the same density. But even this would be an unsafe assumption, for it is by no means likely that two planets of very different mass, even though of similar constitution, would be of the same mean density when passing through the same stages of planetary existence. But by this second method we should probably so far approach the truth as to obtain a fair idea of the relation between the lifetimes of two planets differing in size.

Thus, we could not safely say that because the diameter of Jupiter is ten times that of the earth, therefore each stage of the lifetime of Jupiter exceeds tenfold the corresponding stage of our earth's life. Nor can we say, though this might be nearer the mark, that because Jupiter has a mass very nearly equal to seven-times seven-times seven-times the mass of the earth, therefore each stage of Jupiter's life is seven-times as long as the same stage of the lifetime of the earth. But though neither of these results may be even near the truth, the second probably gives a fair idea of the enormous difference between the duration of each stage through which our earth has passed, and the duration of the same stage in the lifetime of Jupiter, whether he has already passed through such stage, or still has to do so.

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[1] Thus the volumes are as the cubes or third powers of the diameter, while the surfaces are as the squares; so that, since the quantities of heat are as the volumes, and the ratio of parting with the heat is as the surfaces, it follows that the durations of the cooling must be as the cubes divided by the squares (these divisions relate only to the numbers representing cubes and squares) or as the diameters directly.

Now take the numbers which such a comparison introduces, in the case first of an orb like Jupiter, very much more massive than the earth, and then in that of an orb of very much smaller mass, like our moon.

It has been shown that had past geological changes in the earth taken place at the same rate as those which are now in progress, one hundred millions of years at the very least would have been required to produce those effects which have actually been produced, we find, since the earth's surface was fit to be the abode of life. But recently it has been pointed out, correctly in all probability, that under the greater tide-raising power of the moon in past ages, these changes would have taken place more rapidly. As, however, certainly ten millions of years, and probably a much longer time, must have elapsed since the moon was at that favourable distance for raising tides, we are by no means enabled, as some well-meaning but mistaken persons have imagined, to reduce the life-bearing stage of the earth from a duration of a hundred millions of years to a minute fraction of such a period. The short life, but exceedingly lively one, which they desire to see established by geological or astronomical reasoning, never can be demonstrated. At the very least, we must assign ten millions of years to the life-bearing stage of the earth's existence. If now we multiply this period by seven for Jupiter, we get a period sixty millions of years longer!

But take the stage preceding that of life on the earth.

From the researches of Bischoff into the cooling of masses of heated rock, it seems to follow that a period of more than three hundred millions of years must have been required for the cooling of the earth from a temperature of 2,000 degrees Centigrade to one of 200 degrees, a cooling which has certainly taken place. Suppose, however, that these experiments, or the calculations based on them, were vitiated by some error so considerable as to increase the real duration of the fiery stage of our earth's history more than tenfold, the real duration of that period being only 30 millions of years. Multiply this in turn by 7, and we get a period of 210 millions of years, or 180 millions of years longer.

We ought next to consider the vaporous stage; but the evidence on which to form an opinion as to the duration of this stage of a planet's history is too slight to be the basis of actual calculation. Here, as Tyndall has well remarked, 'conjecture must entirely cease.'

But, by considering only two stages—the fiery stage and the life-bearing, or rather that portion of the life-bearing stage through which the earth has hitherto passed—we find the two monstrous time-differences—180 millions and 60 millions of years, or 240 millions of years in all. They mean that, if our assumption as to the effect of Jupiter's superior mass is correct, then, supposing Jupiter and the earth to have started into existence as distinct orbs at the same or nearly the same time, 240 millions of years must elapse before Jupiter will reach the stage of planetary life through which our earth is now passing. Whether the assumption be correct or not, the time-differences between the stages of Jupiter's life and the earth's are of this order. They must be measured by tens of millions if not by hundreds of millions of years.

We must note, however, that the 240 millions of years correspond with only a seventh part of that time in the earth's history: so that we may say that, if our assumptions are correct, Jupiter would now be in the state in which our earth was 34 millions of years ago, or nearer the beginning than the end of the fiery stage.

Take next a much smaller orb than the earth—the moon, for example, with a mass equal but to the 81st part of hers. Here we may compare the actual masses and surfaces, knowing that when the earth has parted with all her internal heat her mean density will not exceed very greatly that which she has now. (It would strengthen our argument to suppose that she would then be very much denser, or, which amounts to the same thing, that the moon, when in the same stage of planetary life as the earth, was very much less dense than at present.)

The moon's mass is but the 81st part of the earth's, her surface being less than the earth's about as 2 is less than 27. Since the earth, when at any given stage of her career as a planet, had 81 times as much heat as the moon, and since she had a surface not 81 times but only 13½ times greater than the moon's, each past stage of her cooling has lasted, and each of those to come will last longer than the corresponding stage of the moon's cooling, in the same degree that 81 exceeds 13½, or about 6 times. Taking, then, the periods we adopted when comparing the earth and Jupiter —viz. 10 millions of years for the life-bearing stage and 30 millions for the fiery stage—we find that these periods of the moon's history would have lasted 1⅔ millions of years and 5 millions of years respectively. Or, supposing the moon and earth to have begun the fiery stages of their respective lives at the same time, the moon would have attained the earth's present condition after 6⅔ millions of years, or (taking this period from 40 millions of years) 33⅓ millions of years ago. Note further that during these 33 millions of years the moon would have been cooling, not at the earth's rate but at her own much more rapid rate, or six times as fast. They would correspond, therefore, with six times 33⅓ millions of years, or 200,000,000 years in the history of the earth.

We see, then, that, while we might expect to find Jupiter (as we have seen) in an earlier part of the fiery stage of planetary life, we might reasonably expect to find the moon very far advanced in planetary decrepitude. Moreover, we see that, as Jupiter is the largest of the planets and the moon the smallest,[1] we may fairly expect to find among the various members of the solar system illustrations of all the stages of a planet's existence, from that through which Jupiter is passing (presumably the fiery stage) to that through which our moon is passing (presumably either the stage of extreme decrepitude or of death).

But is there one orb in the solar system which might be expected to illustrate the first stage of all, the glowing vaporous stage which may be regarded as corresponding with the infancy of planetary existence?

There is one orb so much larger than any other that we might expect him to be far younger even than Jupiter. That orb is the sun, with a diameter exceeding Jupiter's in about the same degree that Jupiter's exceeds that of the earth, with his volume exceeding the earth's a million and a quarter times, with his mighty mass exceeding hers 326,000 times, and Jupiter's not much less than a thousand times. If we applied to him the same reasoning that we have applied to Jupiter, we should expect to find each stage of his tremendous life-existence ten times longer than the corresponding stage in the existence of Jupiter. Setting 40 millions of years as the time since the earth began the fiery stage of her career, and 280 millions of years as the time required by Jupiter to complete the changes which the earth has passed through in those 40 millions of years, we should find for the sun no less than 2,800 millions of years. That is, taking no account of the vaporous stage, in which nevertheless it may be presumed that he still is, it would follow that 2,800 millions of years, less 40 millions, would have to pass before the sun's mass will have cooled to the same degree as the earth's at this present time. We may, then, certainly expect to find the sun in the first or glowing vaporous stage of orb life, with at least as much reason as we found in support of the belief that Jupiter is in the second.

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[1] We except, of course, the asteroids, each of which is far smaller than the moon. The satellites of other planets need not be excepted; for not one of them can, like the moon, be regarded as a planet. They really are dependent bodies, utterly small and insignificant compared with the orb round which they travel. With the moon it is different, she may justly be described as our earth's companion planet. A modern astronomer has described her as a planet travelling round the sun and largely perturbed by the attractions of a neighbouring planet—the earth.

Now let us briefly consider whether the characteristic features which we have seen to belong to the five stages of an orb's life are really shown by members of the solar system which, judging from their dimensions, we might expect to find in those stages of life respectively.

Turn first to the sun. We need no telescopic scrutiny to assure us that he at least presents two of the characteristics assigned to an orb in the glowing vaporous state. He unquestionably pours forth enormous quantities of heat, he assuredly glows with an intense lustre. The glory of the sun and his fiery heat assure us as with the clearest words that he is in the infancy of his career as an orb in space. When, however, we examine his structure with the telescope, and, taking the aid of that wondrous instrument of research, the spectroscope, ascertain the very materials of which his glowing mass is formed and determine their condition, we have even clearer evidence. His lustre and his heat, however accordant with what analogy would have led us to expect, might be otherwise explained than by assuming that he is simply an orb like our earth, but very much larger and in a much earlier stage of existence. He might be made of very different materials, and his lustre and his heat might be produced in a way entirely differing from any known way by which on this earth heat and light are generated. But we know him, from closer scientific scrutiny, to be made of the same materials as the earth, we know those materials to be in the vaporous condition which only intense heat will produce, and we see that the whole of the sun's glowing mass is disturbed in such a way as to indicate that the greater part of that mass can be in no other than the vaporous state. So also, when we determine the mean density of the sun, we find that, instead of being far more compressed than the earth, as we might expect from his enormous mass were he in the same condition, he has a mean density only about one-fourth of hers.

Then outside the visible sun we see the mighty flames which leap over his surface to a height of thirty, forty, fifty, sometimes even eighty or a hundred thousand miles, great masses of glowing gas shot forth from his interior with velocities so great that compared with them all forms of motion with which we are acquainted on earth seem absolutely as rest. Outside these again are the mystic streamers of the corona, extending to distances of two hundred, three hundred, sometimes five hundred thousand, or even a million miles. All these features, the wonders of which might well occupy our thoughts during a longer time than all the rest that we have thus far considered, show that the sun is in reality an orb in the first stage of world life, the glowing vaporous stage, when the whole frame of an orb is instinct with fiery heat and aglow with intense lustre.

Turn next to the planet which we have regarded as likely to illustrate the second or fiery stage of a world's life—the planet Jupiter. Or consider, not Jupiter alone, but the brother giants, Jupiter and Saturn.

When we examine these planets with the telescope we find that every feature of their disc corresponds with the theory that the real surfaces of both these planets are hidden beneath deep cloud masses. We find abundant traces of intense disturbance affecting these cloud masses. And further, when we consider the mean density either of Jupiter or of Saturn, we find the most decisive characteristic of all—the small mean density, which we expect to recognise in a planet in the fiery stage, but which certainly would not be shown by the giant masses of Jupiter and

Saturn, unless there were some powerful forces at work counteracting the tremendous attractive influence of the matter composing their tremendous globes. Were Saturn or Jupiter in the same condition as the earth, as well as made (which we may well believe to be the case) of the same materials, we know that the tremendous pressure to which their substance would everywhere be exposed would result in producing a density far greater than that of the earth with her much smaller mass and relatively much smaller attractive forces. But so far is this from being the case, that the density of Jupiter is but one-fourth, that of Saturn little more than one-seventh, the mean density of our earth.

In the case of the giant planets, then, as in that of the still vaster orb of the sun, we find what theory had led us to anticipate. We find that these orbs coming next in order, in point of size, to the sun, present all the appearance of being in the next stage of orb life. Just as he presents all the characteristics which we expect to find in orbs in the first or glowing vaporous stage of existence, so do these orbs present all the characteristics of the second or fiery stage.

Passing over the third or middle stage, in which we know our earth to be, let us seek for an illustration of the fourth stage, or the stage of planetary decrepitude. We have seen reason to believe that our moon, so much less than the earth in size and mass, is probably either in a state of advanced decrepitude, or actually dead. If there is any orb between the earth and the moon in size and mass much less than the earth and much larger than the moon, we might fairly expect to find that orb in the middle state between the earth and the moon, not so old as the moon, but still a great deal older than the earth.

The planet Mars is such an orb. The moon has a mass equal to the 81st part of the earth ; Mars has a mass equal to nearly one-ninth of the earth's, or nearly nine times as great as the moon's. So that, precisely as the sun exceeds

Jupiter in mass in about the same degree that Jupiter exceeds the earth, so Mars exceeds the moon in mass in about the same degree that the earth exceeds Mars. We could not therefore have, on *à priori* grounds, a more convenient middle term than Mars, for the stage midway between those through which the earth and the moon are respectively passing.

Now, when we make comparison between the earth and Mars in that feature which we have regarded as characteristic of advanced age, what do we find? On the earth the water surface is equal to about three-fourths of the entire surface of the globe ; in Mars the surface occupied by water is but about one-half (rather less, according to the observations of some astronomers). Again, when we examine the seas of Mars in respect to their shape, we find precisely the feature which we were led to expect in a planet which had passed so far beyond the stage in which the earth now is that the seas had become greatly reduced in extent. We find seas like the Baltic and the Mediterranean as characteristic in the case of Mars as they are unusual in the case of the earth. Nine of the seas on Mars have this peculiar shape, which Stanislas Meunier, following myself, has described as bottle-necked.

So far as we can judge, in fine, all the features of Mars, like those of the sun and Jupiter, correspond with the anticipations which we should have based on the planet's mass.

Lastly, we turn to the moon. This planet, we have seen, should be in the state which the earth will reach 200 millions of years hence, if our assumptions as to the duration of the various stages of the earth's existence are correct.[1]

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[1] It is worth noticing that in reality it matters very little whether these assumptions are correct or not, so far as our ideas respecting the stage of life reached by the several planets are concerned. If we have over-estimated the duration of the various stages of the earth's existence, we have correspondingly under-estimated the rate at which the changes which characterise these various stages proceed, so that if we have in consequence over-estimated the difference of the durations of the stages

As we have said, it might fairly be expected that at that remote date the earth will be dead, in the sense of being unfit any longer to support life. The moon, then, if our theory be correct, might be expected to be a dead world. Now, what do we find? So far as the disappearance of seas and oceans is concerned, the moon certainly presents every sign of death. There are astronomers, indeed, who consider that there are traces of moisture on the moon, and some few observers (who certainly are *not* astronomers) who consider that they can trace signs of water-surfaces—small pools and so forth—on the moon. But, as a matter of fact, all astronomers of repute agree that the surface of the moon is absolutely arid—dry, desolate, and dead. Again, so far has the moon progressed towards the final stage of planetary existence, even if she have not absolutely reached it, that no trace whatever of an atmosphere can be recognised on her surface. As she passes over the stars, she obliterates them from our view, with a suddenness which shows that either she has no atmosphere or an atmosphere so tenuous as to have no appreciable effect in softening the edge of the lunar disc, so as to diminish in any perceptible degree the suddenness with which stars are obliterated from our view. There is no trace of any twilight on the moon. The shadows of mountains are black and sharply defined, instead of presenting those half-tints which we know to exist in the shadows of our own mountains. When, standing on some mountain summit at sunrise or sunset, we look into the shadowed valleys, we do not look into absolute blackness; and if we ask where the light comes from which partially

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of these planets' lives, the changes taking place during such over-estimated period will not be greater than those really due to the difference correctly estimated, for they will take place at a correspondingly slower rate. And *vice versâ*, if we have under-estimated the difference of the durations of the stages of other planets' lives, the changes taking place during such under-estimated periods will not be less than those really due to the differences correctly estimated, for they will take place at a correspondingly quicker rate.

illuminates those valleys, we find our answer at once. We know what we should recognise if we walked into those valleys. We should find light reaching us from the sky. The valleys could not be in black shadow unless the skies were also black. Seeing, then, that the shadows of the lunar mountains are absolutely black, we learn as certainly as we should if we could actually have visited the moon and walked into those shadowed regions, that the sky seen overhead there would be absolutely black, with the stars shining as brilliantly there in broad daylight as they do with us on the clearest and darkest night. Now, our sky is in reality illuminated air—air which, receiving the solar rays, refracts and reflects it towards the earth, so that to say the sky is black as seen from the moon is equivalent to saying that either there is no air, or the air is so thin that it reflects no perceptible portion of the solar rays.

Waterless, then, and airless, the moon must be regarded as dead. She tells the same story as the sun, the giant planets, our earth, and Mars. She is in the stage corresponding to her size.

We have then, in the solar system, examples of all the five stages of a planet's history—the first or glowing vaporous stage, the second or fiery stage, the stage of mid-life, the stage of decrepitude or old age, and finally the stage of death, when no life can any longer be borne on the planet's dry, desolate, and dead surface.

THE SUN AS A PERPETUAL MACHINE.

Among the problems which have proved most perplexing to astronomers and physicists, there are few which surpass in difficulty the problem of the conservation of solar energy. The mighty orb of the sun pours forth in each second of time as much heat as would come from the burning of 16,436 millions of millions of tons of the best anthracite coal. Yet of all this tremendous radiation of heat all the planets together receive less than one 230,000,000th part. When we consider this it seems at first view as though there were some degree of truth in the saying that in the universe 'we find Nature upsetting a gallon to fill a wine-glass.'

In company with this great mystery of seeming waste comes the yet more difficult problem, How to explain the apparent continuance of solar light and heat during millions of years. We know from the results of geological research that the earth has been exposed to the action of the solar rays with their present activity during at least a hundred million years. Yet it is difficult to see how on any hypothesis of the generation of solar heat, or by combining together all possible modes of heat-generation, a supply for more than 20 millions of years in the past and a possible supply for as long a period in the future can be accounted for.

It is well known, of course, to all who are likely to read these lines that Dr. Siemens is the inventor of what is called the regenerative furnace, in which the heat, which in ordinary furnaces goes up the furnace chimney and is wasted, is carried back and made to do work. His theory of the solar heat seems to have been suggested by this invention of his own. The enormous waste of solar energy which unquestionably takes place if those rays which do not fall on planets do not do their proper work is obviated, he believes, by a contrivance (if one may so speak) which enables them to store up work in interstellar space, which is presently brought back to its source for fresh use. According to this view, and it is this which renders the theory attractive to many who had been appalled by the seemingly wanton waste of all save the minutest fraction of the sun's heat, only those rays which fall on the planets are actually and finally used up, so that, if the theory be true, the supply of solar heat will last 230 millions of times longer than it otherwise would. Moreover, the theory has its retrospective side. The difficulty about the past would be removed as completely as what had seemed a danger in the future. If the theory is correct we may multiply every year during which it had been calculated that the supply has continued by 230 millions, to obtain a rough approximation to the time during which the sun has actually been at work at his present rate of emission.

In the first place we are to assume that the gaseous atmospheres surrounding the sun and the planets are not limited, as Wollaston and others have supposed, but extend to indefinite distances, though of course in a very attenuated condition. 'Following out the molecular theory of gases as laid down by Clerk Maxwell, Clausius, and Thomson,' says Dr. Siemens, 'it would be difficult to assign a limit to a gaseous atmosphere in space; and further, some writers, among whom I will here mention only Grove, Humboldt, Zöllner, and Mattieu Williams, have boldly asserted the existence of a space filled with matter, and Newton himself, as Dr. Sterry Hunt tells us, has expressed views in favour of such an assumption.' He proceeds to notice the evidence in favour of this view derived from the condition in which meteorolites reach the earth. They are known, he says, to contain as much as six times their own volume of gases (taken at atmospheric pressure). In one of these meteorolites recently examined by Dr. Flight, the following percentages of various gases were noted. Of carbonic oxide 31·88, of carbonic acid gas 0·12, of hydrogen 45·79, of olefiant gas 4·55, and of nitrogen 17·66. Here, however, I may note in passing that although it is quite certain these gases were not taken up by the meteorolite during its flight through our air, it by no means follows, and is indeed exceedingly improbable, if not impossible, that they were taken up while the meteorolite was travelling freely through interplanetary or interstellar space. The general belief is that, as the late Professor Graham aptly expressed it, these bodies bring to us the hydrogen of the fixed stars (including our own sun)—that, in fact, they were expelled from bodies in a state resembling our sun, and that during their abode within the intensely hot orb of their parent sun, the hydrogen and other gases which we know to exist in the sun and his fellow stars were forced into (or became occluded in) the substance of the mass which was afterwards to become a meteorolite, and after long and devious wanderings to reach our earth. Thus, and thus only it is believed by chemists, can the enormous quantity of occluded hydrogen in the substance of meteors be explained; for nowhere else, but in the interior of suns, is there either the necessary heat or the necessary pressure. The absence of any trace of aqueous vapour, which Dr. Siemens finds surprising, as indeed it is on his theory, is thus readily accounted for; indeed, no one would expect to find aqueous vapour in the substance of a meteoric mass which had ever had its abode in the interior of a sun.

Dr. Siemens considers the objection that if interplanetary space were occupied by gases, the planets would be seriously retarded. He believes that, assuming the matter occupying space to be an almost perfect fluid not limited by border surfaces, the retardation by friction through such an attenuated medium would be very slight indeed, even on bodies moving with planetary velocities. But in this he is altogether mistaken.

He notes also another objection, namely, that if the theory of gaseous interplanetary matter were true, the sun should draw to himself the greater part of the heavier gases, such as carbonic acid gas (carbonic anhydride), carbonic oxide, oxygen and nitrogen; whereas spectroscopic analysis indicates at least the much greater prevalence of hydrogen, if not the absolute absence of these gases. Oxygen, indeed, has been shown by Dr. Draper to be present in the sun. Dr. Siemens points out that at the tremendous heat of the sun's mass such compound gases as carbonic oxide and carbonic acid could not exist as such. But he says that there must be regions, outside the intensely heated regions, where the existence of these gases would not be jeopardised by heat; and in these regions accumulation of these comparatively heavy gases would take place 'were it not for a certain counterbalancing action.'

And here we approach what Dr. Siemens describes as a point of principal importance in his argument, upon the proof of which his further conclusions must depend.

The sun rotates on his axis, completing one revolution in about twenty-five days, and 'the sun's diameter being taken at 882,000 miles' (it is really considerably less than this, however), 'it follows that the tangential velocity amounts to 1·25 miles per second, or to 4·41 times the tangential velocity of our earth. This high rotative velocity of the sun must cause' (it is Dr. Siemens who speaks) 'an equatorial rise of the solar atmosphere to which Mairan, in 1731, attributed the appearance of the zodiacal light.' He goes on to consider Laplace's objection to this explanation on the ground that the zodiacal light extends to a distance from the sun exceeding our own distance, whereas the equatorial rise of the solar atmosphere due to its rotation could not exceed 9-20ths of the distance of Mercury. But Dr. Siemens finds in the existence of a medium of unbounded extension an answer to Laplace's objection. 'In this case,' he says, 'pressures would be balanced all round, and the sun would act mechanically upon the floating matter surrounding it, in the manner of a fan, drawing it towards itself upon the solar surfaces, and projecting it outwards in a continuous disc-like stream.'

Now it is just at this critical part of the theory, on the proof of which the further conclusions of the theorist must depend, that dynamical considerations throw doubt, and something more than doubt, upon the entire speculation.

We have a supposed fan-like action, by which hydrogen, hydro-carbons, and oxygen are supposed to be drawn in enormous quantities towards the polar surface of the sun. During their approach they are supposed to pass from their condition of extreme attenuation and extreme cold, to that of compression, accompanied with rise of temperature, until on approaching the photosphere they burst into flame, giving rise to a great development of heat, and a temperature commensurate with their point of dissociation at the solar density. The result of their combustion is aqueous vapour and carbonic acid or carbonic oxide, according to the sufficiency or insufficiency of oxygen present to complete the combustion, and these products of combustion in yielding to the influence of centrifugal force will flow towards the solar equator. . . . *So much* we may regard as possible, though much would have to be proved before it could be regarded as probable. But Dr. Siemens goes on to say that the matter thus carried towards the solar equator *will be thence projected into space.*

Now there can be nothing simpler than the considerations on which such projection into space would depend. The question whether a body moving in a particular way at any part of the sun's surface will travel outwards into space, or will not travel outwards, can be answered according to certain very definite laws. If the velocity of its motion exceeds a certain amount, the body will recede from the sun; if it falls short of that amount the body will tend to approach the sun's centre; if the body has just that velocity, then the body will neither recede nor approach. Now it suggests the idea of tremendous centrifugal tendency to say that at the sun's equator the velocity is 4·41 times the tangential velocity (at the equator) of our earth. Bodies do not fly from our earth's equator on account of the enormous tangential velocity there (more than a thousand miles per hour); but it is easy to imagine, as Dr. Siemens evidently does, that with the much greater velocity at the sun's equator there may be such a tendency as his theory requires. What is, however, the actual state of the case? Centrifugal tendency varies in the first place as the square of the velocity; and squaring 4·41 we get 19·45; so that if our earth were to rotate 4·41 times as fast as she actually does, the centrifugal force at the equator would be increased 19·45 times. Even that would not be nearly enough to make bodies fly off at the equator. (In fact it can easily be shown that for bodies just to become weightless at the equator the earth should rotate in 1⅓ hours, or *sixteen* times as fast as at present.) But this is only a small part of the matter. Centrifugal force not only varies as the square of the velocity, but inversely as the distance from the centre of motion. So that as the sun's diameter exceeds the earth's about 108 times, centrifugal tendency at his equator is diminished in this degree so far as this particular circumstance is concerned. Increasing the tendency 19·45 times and reducing it 108 times, means in all reducing it to about two-elevenths of the centrifugal tendency at the earth's equator. Yet even this is not all. Not only is the centrifugal tendency at the sun's equator less than a fifth that at the earth's equator, which diminishes by a very small part the force of terrestrial gravity, but the centripetal tendency due to the sun's attractive force is very much greater at the sun's surface than terrestrial gravity at the earth's equator. It is roughly about twenty-seven times as great. Thus the centripetal tendency of matter at the sun's equator is very much greater (many hundreds of times greater) than its centrifugal tendency;

and there is not the slightest possibility of matter being projected into space from the sun's surface by centrifugal tendency. Nor is there any part of the sun's mass where the centrifugal tendency is greater than at the surface near the equator. So that whatever else the sun may be doing to utilise his mighty energies, he is certainly not throwing off matter constantly from his equatorial regions, as Dr. Siemens' theory requires.

This being so, the theory failing thus in a matter absolutely essential to its validity, we may feel less tempted than perhaps we otherwise might be, to endeavour to overlook other difficulties, though these on careful consideration appear scarcely less decisive. It might perhaps appear a work of supererogation to consider difficulties when we have already noted an impossibility. But some perhaps will consider that although the sun may not, after drawing to himself the matter occupying space, reject it from him in the manner supposed, he may reject it in some other manner. If so there might still be reason for inquiring how far it is likely that the sun's rays may be utilised when falling on the matter occupying space, in the way suggested by Dr. Siemens.

Let us then grant the existence in interplanetary space of those products of combustion which Dr. Siemens supposes to be constantly projected from the sun, and let us inquire with him what would become of them. At a first view it seems as though they must gradually change the condition of the matter which had formed part of stars and suns, by rendering that matter neutral. But Dr. Siemens endeavours to show the possibility, nay, the probability, that solar radiation would under these circumstances step in to bring back the combined materials to a condition of separation by a process of dissociation, carried into effect at the expense of that solar energy which is now supposed to be lost to our planetary system.

Dr. Siemens points out that the temperature at which the dissociation of different compounds is effected depends on the pressure. Thus at a temperature of 2,800° Centigrade only one-half of the vapour of water at atmospheric pressure remains as aqueous vapour, the remaining half being found as a mechanical mixture of hydrogen and oxygen. But with the pressure the temperature of dissociation rises and falls. It is therefore conceivable, he says, that the temperature of the solar photosphere may be raised by combustion to a temperature exceeding 2,800° Centigrade, whereas in interstellar and interplanetary space dissociation may be effected at a much lower temperature. Some experiments by Dr. Siemens appear to show that at the small pressure which we may conceive to exist in space, the sun's radiation may suffice to produce dissociation either of aqueous vapour or of carbonic acid gas. Employing glass tubes furnished with platinum electrodes, and filled with aqueous vapour, he reduced the pressure to $\frac{1}{1800}$th of an atmosphere, the temperature being reduced to 32° Centigrade. When so cooled, no electric discharge took place on connecting the two electrodes with a small induction coil. He then exposed the end of the tube projecting out of the freezing mixture, backed by white paper, to solar radiation on a clear summer's day for several hours, when upon again connecting up to the inductorium, a discharge, apparently that of a hydrogen vacuum, was obtained. 'This experiment being repeated, furnished,' says Dr. Siemens, 'unmistakable evidence I thought that aqueous vapour had been dissociated by exposure to solar radiation.' When carbonic acid gas was similarly treated, less trustworthy results were obtained. 'Not satisfied with these qualitative results, I made arrangements to collect the permanent gases so produced, by means of a Sprengel pump, but was prevented by lack of time from pursuing the inquiry, which I purpose, however,' adds Dr. Siemens, 'to resume shortly, being of opinion that, independently of my present speculation, the experiments may prove useful in extending our knowledge regarding the laws of dissociation.'

The idea is, then, that solar radiation acting on the aqueous vapour and carbonic acid gas, and other compound gases supposed to occupy interplanetary and interstellar space, may dissociate such compounds, and that solar energy may thus be utilised, instead of being wasted in the enormous degree in which it appears to be, according to what has been shown above.

Now it appears to me somewhat bold to assume that what happens in the case of aqueous vapour or carbonic acid enclosed in a tube and exposed to solar radiation, would happen to such vapour exposed to the same radiation in free space. But there is a more serious objection, I take it, than this, to Dr. Siemens' ingenious system for the utilisation of solar energy. If the rays of heat (and light) are thus utilised within the solar domain, regarding that if we please as extending many times further than the orbit of Neptune, they have either done their work and have been completely utilised, or they have not. If they have done their work, these rays proceed no further, and the sun would therefore be invisible from any point outside his own domain. (For we must not fall into the mistake of supposing that light and heat can be considered separately in this inquiry : those solar rays which give us what we call light, give us also a large quantity of the solar heat, and the mystery of seemingly infinite waste would remain, even if we supposed that only those heat rays which are not also light rays were utilised in the way supposed. Apart from this, Dr. Siemens specially shows how the light rays act in accordance with his views.) Now what is true of our sun is true of the other suns, the stars. They also ought to be invisible outside their several domains. But as a matter of fact they are visible. If, on the other hand, the solar rays have *not* done their work in traversing what may be regarded as the solar domain, the mystery of infinite waste is not removed, scarcely even diminished, by Dr. Siemens' theory. If those other suns, the stars, are able to send across the vast distances which separate us from them, such supplies of light (to say nothing of stellar heat, which Huggins and others have measured) that by measuring it we can say that all of them are suns like our own, but many far larger and giving out much more light than he,—what is the amount of work which we can suppose the stellar rays to have done on their way? If they have done much (in proportion to the total quantity which they are capable of doing), then the stars must be very much larger, brighter, and hotter than we suppose them to be, and already we regard them as the rivals, and something more than the rivals, of our sun. If they have done little, the mystery of infinite waste remains.

But indeed, apart from the considerations last urged, it is certain that even if the whole of interstellar space were filled with matter dissociated by solar rays (that is by the rays which all suns are continually pouring forth), even then those rays would have been to all intents and purposes wasted ; for suns never could gather in more than the minutest fraction of the matter thus permeating space. We cannot adopt Dr. Siemens' theory, supposing it otherwise tenable, as a means of utilising solar and stellar energy, unless we supposed the work done by the light and heat of suns to be done close to those orbs, certainly far within the orbits of their outer planets, for otherwise the matter prepared for fuel by the action of the rays could never be gathered in, or the products of combustion expelled, within reasonable time, throughout the domain thus affected. But we know certainly that within such relatively insignificant domains the stellar rays are not used up, for we see the stars shining, though we lie millions of times farther away than any conceivable limits of such domains. We know it in the case of our own sun, because we see the planets Saturn, Mars, and Neptune shining with light which has reached them from the sun. In the case of the Siemens' regenerative furnace, we know that the heat is utilised in the particular manner intended, not only because we find the heat so saved doing its proper work, but because we find that this heat no longer goes idly up the furnace chimney as before. The heat cannot be doing its full work in the furnace if part goes up the furnace chimney; but also, part cannot be going up the furnace chimney if the heat is doing its full work. This, however, is what Dr. Siemens' theory requires the solar heat to do. It is to be continually utilised in dissociating compound vapours in interplanetary space, although it is continually passing beyond interplanetary space to shine through interstellar space, and to show our sun as a star to worlds circling round his fellow-stars the suns. We have in fact the fallacy of the perpetual motion in a modified form.

Parts of Dr. Siemens' reasoning remain tenable, however, even when the centrifugal projective force (which has no existence) is removed, and when the perpetual utilisation of stellar rays is shown to be inconsistent with their perpetual passage with undiminished brightness through interstellar space.

Dr. Siemens' reasoning respecting the zodiacal light, for instance, is sound, though the theory with which it is associated is not so. Astronomers do not and cannot accept the views of Mairan, which are simply inconsistent with the known laws of dynamics. But there is every reason for regarding the zodiacal as consisting in the main of meteorolithic masses, a sort of cosmical dust, rushing through interplanetary space with planetary velocities. To such matter, assuming, as we well may, that space really is occupied by attenuated vapours, the following reasoning applies with scarcely the change of a word (by which, however, I do not mean that the opinions expressed as probably or possibly true are really and necessarily so). The luminosity of the zodiacal 'would be attributable to particles of dust, emitting light reflected from the sun, or by phosphorescence' (this last may be seriously questioned). 'But there is another cause for luminosity of these particles, which may deserve a passing consideration. Each particle would be electrified by gaseous friction in its acceleration, and its electric tension would be vastly increased in its forcible removal, in the same way as the fine dust of the desert has been observed by Werner Siemens to be in a state of high electrification on the apex of the Cheops Pyramid. Would not the zodiacal light also find explanation by slow electric discharges backward from the dust towards the sun?'

Take, again, the phenomena of comets which still remain among the greatest of nature's mysteries. We have reason to believe—though Dr. Siemens goes a little beyond the truth in saying astronomical physicists *assert*—that the nucleus of a comet consists of an aggregation of stones similar to meteorolites. Adopting this view, and assuming that these stones have absorbed somewhere (not necessarily 'in stellar space,' as Dr. Siemens suggests) gases to the amount of six times their volume (taken at atmospheric pressure), we may ask with Dr. Siemens, what will be the effect of such a mass of stone advancing towards the sun at a velocity reaching in perihelion the prodigious rate of 366 miles per second (as observed in the comet of 1843), being twenty-three times our orbital rate of motion? 'It appears evident that the entry of such a divided mass into a comparatively dense atmosphere must be accompanied by a rise of temperature by frictional resistance, aided by attractive condensation. At a certain point the increase of temperature must cause ignition, and the heat thus produced must drive out the occluded gases, which in an atmosphere 3,000 times less dense than that of our earth would produce ($6\times3{,}000=$)18,000 times the volume of the stones themselves. These gases would issue forth in all directions, but would remain unobserved except in that of motion, in which they would meet the interplanetary atmosphere with the compound velocity and from a zone of intense combustion, such as Dr. Huggins has lately observed to surround one side of the nucleus, evidently the side of forward motion. The nucleus would thus emit original light, whereas the tail may be supposed to consist of stellar dust rendered luminous by reflex action produced by the light of the sun and comet combined.' (This assumption respecting the tail is, however, untenable, being based on a misapprehension of the distinction between a comet's tail and its train of meteoric attendants.)

These views respecting the zodiacal light and comets are independent in the main of those parts of Dr. Siemens' views which are manifestly inadmissible. They seem to accord well with possibilities if not with probabilities.

A similar remark applies to two of the fundamental conditions of Dr. Siemens' ingenious theory. We may admit the possibility that the aqueous vapour and carbon compounds are present in stellar or interplanetary space; we may concede, though perhaps not quite so readily, that these gaseous compounds are capable of being dissociated by radiant solar energy while in a state of extreme attenuation. What we cannot admit, simply because it is inconsistent with known laws, is the third condition, 'That these dissociated vapours are capable of being compressed into the solar photosphere by a process of interchange with an equal amount of reassociated vapours, this interchange being effected by the centrifugal action of the sun itself.' As this condition is essential to the theory itself, we are compelled, regretfully perhaps, but still unhesitatingly, to give up that satisfaction which, as Dr. Siemens remarks, we should gain, could we believe that our solar system need 'no longer impress us with the idea of prodigious waste through the dissipation of energy into space, but rather with that of well-ordered, self-sustaining action, capable of perpetuating solar radiation to the remotest future.' Yet though not in this way, to this end all thoughtful study of the mechanism of the universe seems unquestionably to tend; not by centrifugal tendencies of the kind imagined, for none such exist; not by work which, viewed in reference to the universe as we know it, means endless production without exhaustion; but in other ways (associating perhaps our visible universe with others, permeating it as the ether of space permeates the densest solids, and in turn with others so permeated by it) there may be that constant interchange, that perpetual harmony, of which Goethe sung—

> See all things with each other blending,
> Each to all its being lending,
> Each on all in turn depending:
> Heavenly ministers descending,
> And again to Heaven uptending,
> Floating, mingling, interweaving,
> Rising, sinking, and receiving—
> Each from each, while each is giving
> On to each, and each relieving
> Each—the pails of gold. The living
> Current through the air is heaving;
> Breathing blessings see them bending,
> Balanced worlds from change defending,
> While everywhere diffused is harmony unending.

THE SUN'S CORONA.

One after another the mysterious problems presented by the sun to man's contemplation have been solved by astronomers. We have learned what are the substances which compose his giant bulk. We know much respecting the condition in which those substances exist. The strange red prominences which are seen round the black disc of the moon in total eclipse, 'like garnets round a brooch of jet,' have not only been interpreted, but our astronomers, calling in to their aid the subtle powers of the most wonderful instrument of research yet devised by man, have been enabled to discern these objects when the sun is shining with full splendour in the heavens—nay, even to measure their motion, and to gauge the pressure exerted by the gases which compose their substance. But one great problem yet remains unsolved. When the sun's orb is hidden in total eclipse, there bursts suddenly into view a crown or glory of light, resembling the *nimbus* which painters place around the heads of saints. Sometimes presenting the appearance of a uniform circular halo, at others radiated and even irregular in aspect, this striking phenomenon had long attracted the attention and invited the curiosity of astronomers. But recently, owing to the nature of the information obtained respecting the sun's substance and the coloured flames which play over his surface, the corona has been regarded with much greater interest.

The corona was known to astronomers long before those coloured prominences which have recently received so much attention. It has even been supposed that Philostratus refers to the appearance of this object where he remarks, in his 'Life of Apollonius,' that 'there appeared in the heavens'—shortly before the death of Domitian—'a prodigy of the following nature—a certain *corona*, resembling the iris, surrounded the orb of the sun and obscured his light.' One might conceive that there was no reference here to a total eclipse of the sun; but Philostratus remarks farther on, that the darkness was like that of night, a circumstance which leaves little doubt that a solar eclipse had taken place.

It is, in fact, worthy of remark, that the light of the corona often misled the observers of total eclipses to suppose that, in reality, a portion of the sun had remained uncovered. Kepler was at the pains to write a treatise to prove that certain eclipses, supposed to be only annular, had, in reality, been total. A year after he had published this treatise, he himself had an opportunity of witnessing the total eclipse visible at Naples in 1605, respecting which he remarks, that 'the whole body of the sun was completely covered for a short time, but around it there shone a brilliant light of a reddish hue and uniform breadth, which occupied a considerable part of the heavens.'

From this time scarcely a singe total eclipse has occurred during which the aspect and dimensions of the corona have not been noted. It would be easy to fill a volume with the various observations which have thus been recorded. For our purpose, it will be convenient to select those accounts which indicate the most important peculiarities of the corona, and especially those which may help us to ascertain the real nature of the object.

One of the earliest accounts of this nature is that given by Dr. Wyberd of the total eclipse of March 29, 1652. 'When the sun was reduced to a narrow crescent of light,' he remarks, 'the moon all at once threw herself within the margin of the solar disc'—(a peculiarity which has been observed under favourable circumstances by others, and is, of course, only apparent)—'with such agility, that she seemed to revolve like an upper millstone, affording a pleasant spectacle of rotary motion. In reality, however, the sun was totally eclipsed, and the appearance was due to a corona of light round the moon, arising from some unknown cause. It had a uniform breadth of half a digit or a third of a digit at least; it emitted a bright and radiating light, and appeared concentric with the sun and moon' when the centres of the two discs were at their nearest.

It will presently be seen that the extent of the corona on this occasion was far less than during many modern eclipses; in fact, Dr. Wyberd's account would seem to indicate that he only noticed the brighter part of the corona which lies close by the black disc of the moon. Otherwise the extent of the corona on this occasion was exceptionally small. Strangely enough, the next account we have to refer to assigns to the corona an exceptionally large extension from the sun.

During the eclipse of May 12, 1706, MM. Plantade and Capiés saw a very bright ring of white light surrounding the eclipsed sun, and extending to a distance equal to about a tenth of the moon's apparent diameter. This was, in all probability, that brighter portion of the corona which Dr. Wyberd saw. Outside this brilliant ring of light a fainter light was seen, which faded off insensibly until—at a distance from the sun equal to about eight times his apparent diameter—the light was lost in the obscure background of the sky.

This observation serves very well to indicate the interest and importance attaching to the solution of the problem presented by the corona. We shall see presently that a question exists whether the corona is, on the one hand, a solar appendage, or, on the other, a phenomenon due merely to the passage of the sun's rays through our own atmosphere. The observation just described would, in the one case, indicate that the object has a real extension enormously exceeding that of any known celestial object—save perhaps the tails of certain comets—while in the other case, the corona would have no more scientific importance than those long radial beams formed by the light of the sun shining through a bank of clouds. Enormous as is the bulk of the sun—so enormous that the earth on which we live sinks into utter nothingness by comparison—the actual extent of space filled by the coronal light, on the former supposition, could exceed the volume of the sun more than two thousand times!

It is not without some little shame that astronomers refer to the great total eclipse of 1715. Although this eclipse was visible in England, and though it occurred in the time of so great an astronomer as Halley, no adequate preparations were made for observing it. Coates, indeed—a practical astronomer, whose observations would have had a high value—was 'oppressed with too much company,' Halley tells us, to pay special attention to the eclipse. Halley himself made a few commonplace notes on the phenomena presented by the totally eclipsed sun, but we learn nothing new from them respecting the corona.

Nor were the French astronomers more energetic in 1724. But one observation made by Maraldi is worth noticing. He perceived that at the beginning of the eclipse the corona was clearly broader on the side towards which the moon was advancing than on the opposite side, while at the end of the eclipse the reverse was the case. This would seem to show that the corona is a solar appendage, since the moon thus seemed to traverse the corona precisely as she traversed the sun.

The observation made by Maraldi was confirmed by several who observed the total eclipse of 1733 in Sweden. A special interest attaches to this eclipse, because instead of being observed only by astronomers, it was watched by a large number of persons invited to the work by the Royal Society of Sweden. As many of those who propose to join the expedition to view the eclipse of next December have decided to direct their attention to the general aspect of the corona, it is interesting to inquire how far such observations are likely to add to our knowledge. In this respect the Swedish narrative is most encouraging. At Catherinesholm, the pastor of Forshem noticed that the ring of light which appeared round the black disc of the moon was of a reddish colour, an observation confirmed by Vallerius, another pastor, who noticed, however, that a considerable distance from the sun the ring appeared of a greenish hue. The pastor of Smoland states that 'during the total obscuration the edge of the moon's disc resembled gilded brass, and that the faint ring around it emitted rays in an upward as well as in a downward direction, similar to those seen beneath the sun when a shower of rain is impending.' The mathematical lecturer in the Academy of Charlestadt, M. Edstrom, observed these rays with special attention, and remarks respecting them that 'they plainly maintained the same position until they vanished along with the ring upon the reappearance of the sun.' On the other hand, the ring as seen at Lincopia seemed to have no rays.

It is important to inquire whether this difference in the aspect of the corona, as seen at different stations, is due to the condition of the air, the eyesight of the observer, or other such causes. For clearly, if the observer at Lincopia saw an object really different from that seen by Edstrom, it would follow that the corona is a phenomenon of our own atmosphere and not a solar appendage. On other occasions a like difference has been recorded in the aspect of the corona as seen at different stations; but we do not remember any observations which seem calculated to resolve the question just suggested, until the great total eclipse observed last year in America. It is easy to see that, whatever theory of the corona we adopt, the condition of the atmosphere might be expected to affect the aspect of the ring. For obviously this would happen if the coronal light is merely due to the illumination of our atmosphere; while, if the light comes from beyond our atmosphere, it would still be brighter or fainter according as the air was more or less clear. The only convincing form of evidence would be such as showed that some peculiarity of figure, noticed when the ring was seen under unfavourable atmospheric conditions, remained recognisable notwithstanding a great increase in the apparent extent of the ring, when seen *at some distant station*, under more favourable circumstances.

Now during the great eclipse of the year 1869, very remarkable evidence was given, fulfilling these very conditions.

In the first place, all the astronomers who observed the eclipse along the whole path of the shadow, from whence it first fell upon America far in the North-west to the point where it left the American continent and fell upon the Atlantic, noticed the singularly quadrilateral aspect of the corona. This was not only observed with the naked eye, but by telescopists; and in one instance photography recorded the peculiarity most satisfactorily. But this four-cornered aspect belonged only to a portion of the coronal light lying relatively close to the sun. The most distant corner of the four lay at a distance from the moon's disc scarcely exceeding half the moon's apparent diameter. Outside the cornered figure lay a faint glare of light, which seemed to most observers to merge uniformly and gradually into the dark tints of the sky far away from the eclipsed sun.

But there was one party of observers who were stationed above those lower and denser regions of the atmosphere which are most effective in obstructing the passage of light, and especially of light so faint as that which comes from the outer parts of the corona. General Myer, Colonel Winthrop, and others ascended to the summit of White Top Mountain, near Abingdon in Virginia, and thence, at a height of some 5,500 feet above the level of the sea, and immersed so much more deeply in the shadow of the moon than the observers at low levels, they had an opportunity of witnessing the imposing phenomena presented during a total eclipse of the sun. The account they give of the corona becomes, under these circumstances, most instructive. 'To the unaided eye,' says General Myer, 'the eclipse presented, during the total obscuration, a vision magnificent beyond description. As a centre stood the full and intensely black disc of the moon, surrounded by an aureola of soft bright light, through which shot out, as if from the circumference of the moon, straight massive silvery rays, seeming distinct and separate from each other, *to a distance of two or three diameters of the lunar disc*; the whole spectacle showing as upon a background of diffused rose-coloured light. . . . *The silvery rays were longest and most prominent at four points of the circumference*—two upon the upper, and two upon the lower portion, apparently equidistant from each other . . . giving the spectacle a quadrilateral form. The angles of the quadrangle were about opposite the north-eastern, north-western, south-eastern, and south-western points of the disc' (an arrangement corresponding precisely with the observations made at lower levels). 'There was no motion of the rays—they seemed concentric.'

Nothing, as it should seem, could be more convincing than the evidence given by this observation. The radial extensions which, to the observer near the sea-level, reached only to a distance from the moon's edge equalling about half the moon's diameter, were recognised at the higher station as rays four times as long. The influence of the atmosphere in blotting out, so to speak, the fainter portions of the corona is thus made manifest, and so far the evidence strongly favours (to say the least) the supposition that the corona is something lying much farther from us than the limits of the earth's atmosphere.

Let us return, however, to the records of earlier eclipses. Strangely enough the next we have to deal with corresponds very closely with the American eclipse as respects the appearance presented by the corona. 'The most remarkable feature exhibited by the corona,' remarks Professor Grant, speaking of the eclipse of February 1766, 'consisted of four luminous expansions, separated from each other by equal intervals.'

The Spanish admiral, Don Antonio d'Alloa, gives an interesting account of the appearance presented by the corona during the total eclipse of 1778. He states that 'five or six seconds after the commencement of the total obscuration, a brilliant luminous circle was seen surrounding the moon, which became more vivid as the centre of that body continued to approach the centre of the sun. About the middle of the eclipse, its breadth was equal to one-sixth of the moon's diameter. There appeared issuing from it a great number of rays of unequal length, which could be discerned to a distance equal to the lunar diameter. It seemed to be endued with a rapid rotatory motion, which caused it to resemble a firework turning round its centre. The colour of the light was not uniform throughout the whole breadth of the ring. Towards the margin of the lunar disc it appeared of a reddish hue; then it changed to a pale yellow, and from the middle to the outer border the yellow gradually became fainter until at length it seemed almost quite white.'

Passing over several intermediate eclipses, we come to the great eclipse of 1842, remarkable on account of the number of eminent astronomers of all nations who took part in observing it.

The most noteworthy feature in the records of this eclipse is the very wide range of difference in the estimates of the extent attained by the coronal ring. M. Petit, at Montpellier, estimated the width of the corona at barely one-fourth of the moon's diameter. Francis Baily—it was during this eclipse, by the way, that the phenomenon known as 'Baily's Beads' was first observed with attention—considered that the corona was about twice as wide. To Otto Struve, the eminent Prussian observer, the corona seemed yet wider, falling little short of the moon's apparent diameter in extension.

It is interesting to notice these discrepancies between the observations of modern astronomers of repute for accuracy and observing skill. It shows that the differences recorded in the aspect of the corona are not due to such errors as unpractised observers might be expected to make. We shall presently see the importance of thus separating truthful from untrustworthy observations.

Arago made a similar observation during the progress of this eclipse. He remarked in one of the brighter portions of the corona, 'a luminous spot composed of jets entwined in each other, and resembling in appearance a hank of threads in disorder.' It is difficult to understand what this may have been. It would almost seem to give evidence in favour of a view recently put forward, that the light of the corona comes from innumerable streams of meteors in the neighbourhood of the sun.

Some of the rays of the corona during this eclipse were estimated by the younger Struve as nearly eight times the moon's apparent diameter in length, the first instance, be it noted, in which a modern observation has confirmed the account given by MM. Plantade and Capiés in 1706.

In 1851 the Astronomer Royal had a second opportunity of observing the solar corona. It affords interesting evidence of the variability in the appearance of this object according to the circumstances under which it is observed, that Airy recognised a distinct difference not merely in the extent but in the figure of the corona on this occasion. He says, 'The corona was far broader than that which I saw in 1842. Roughly speaking, its breadth was little less than the moon's diameter, but its outline was very irregular. I did not notice any beams projecting from it which deserved notice as much more conspicuous than the others, but the whole was beamy, radiated in structure, and terminated—though very indefinitely—in a way which reminded me of the ornament frequently placed round a mariner's compass. Its colour was white, or resembling that of Venus. I saw no flickering or unsteadiness of light. It was not separated from the moon by any dark ring, nor had it any annular structure. It looked like a radiated luminous cloud behind the moon.'

In 1860 the Astronomer Royal again witnessed the phenomena which accompany a total eclipse of the sun; and again, his evidence respecting the corona assigns to it a figure resembling, 'with some irregularities, the ornament round a compass card.'

And now we are approaching, or, rather, we have already reached the era when other modes of research than mere telescopic observation were to be applied to this perplexing phenomenon. In 1860, Mr. De la Rue and the Padre Secchi succeeded in photographing the eclipsed sun; and though but a small portion of the corona is discernible in their photographs, yet it is quite evident, on a careful comparison of pictures taken at stations widely separated, that at least the brighter portion of the corona belongs to the sun. Where the coronal radiance is brightest or extends farthest in Mr. De la Rue's pictures, there also in F. Secchi's can be recognised corresponding peculiarities.

Then, after a considerable interval, came the great eclipse of August 1868, when an effort was made to apply the powers of the spectroscope to the interpretation of the corona. It is a somewhat singular circumstance, by the bye, that the results of so important an observation as Major Tennant's spectroscopic study of the corona should be quite commonly misquoted—but so it is. We have before us, as we write, his own statement, in which are the words (italicised), 'What I saw was undoubtedly a continuous spectrum, and I saw no lines;' followed by the remark, 'there may have been dark lines, of course, but with so faint a spectrum . . . . they might escape notice.' Yet in Roscoe's most valuable treatise on spectrum analysis there occur the words, 'Major Tennant states that the spectrum of the corona is the ordinary solar spectrum;' and the American astronomers who observed the eclipse of 1869 repeat the statement, commenting with surprise on the fact that *they* could see no dark lines in the coronal spectrum.

The distinction between what Major Tennant actually saw and what he is supposed to have seen is most important. If the corona gave a spectrum resembling the sun's, it would be reasonable to conclude that the light of the corona was simply reflected sunlight. But if the spectrum of the corona shows no dark lines we can no longer suppose this. A burning solid gives a rainbow-tinted spectrum of this sort, without dark lines; and though it would not be proved, it would at least be rendered probable, were this the nature of the coronal spectrum, that the light of the corona comes from actually incandescent substances.

It was hoped that the American astronomers would have obtained decisive results; but a new source of perplexity was introduced by their observations. They satisfied themselves that the coronal spectrum really was continuous; for they observed it under conditions which removed all the doubts referred to by Major Tennant. But superposed upon the faint rainbow-tinted streak they saw bright lines. Professor Harkness saw one line only, but Professor Young saw three.

Now, it is only necessary to know what is the interpretation of a spectral bright line to understand the strange significance of this new observation. A glowing vapour gives a spectrum of bright lines. But surprising as the conclusion would be that the corona consists, either wholly or in part, of glowing vapour, it is when we consider the nature of the vapour indicated by the coronal bright lines that the most startling result of all is suggested. One of the bright lines corresponds in place with a line belonging to the spectrum of the glowing vapour of *iron*. This metal, which requires so intense a heat for its liquefaction, and, therefore, a yet more tremendous heat to vaporise it, would actually seem (from the evidence) to be present in the form of glowing vapour in the sun's corona. Here are the words of Professor Harkness—who is thoroughly familiar with the laws of spectroscopic analysis—announcing his acceptance of a conclusion as probable, which is so startling that we could not venture to leave it on record without such confirmation, lest haply the reader should regard it as simply arising from a misinterpretation of the evidence:—'I consider the conclusion highly probable, if not actually proved, that the corona is a very rarefied self-luminous atmosphere surrounding the sun, and, perhaps, principally composed of the incandescent vapour of iron.' And what renders the conclusion so much the more remarkable is that Professor Harkness has adduced evidence to show that the heat of the summits of the coloured prominences is such as would be insufficient to vaporise iron. The corona would be less heated, one would suppose, than the prominences which lie so much nearer to the sun.

Such were, up to the year 1870, the observations which astronomers and physicists had made upon the corona. We have indicated in passing some of the theories suggested by special observations, but we have now to inquire what are the general results to which this series of researches, regarded as a whole, appears to tend.

The theories which have been put forward by astronomers in explanation of the solar corona are not many in number, and some of them need not occupy us for any length of time, as modern researches have practically disposed of them.

The theory that the corona is due to a lunar atmosphere is associated with the names of the eminent astronomers Kepler and Halley. It is probable that the latter would have been even more confident of its truth than he actually was, had it not been that the opinion of his great friend Newton was opposed to this theory. Such, at least, has been the interpretation placed upon Halley's remark that 'the contrary sentiments of one whose judgment he should always revere' caused him to feel doubtful as to Kepler's theory.

We now know quite certainly that the moon has no atmosphere which could account for the appearance of the corona. It is doubtful whether the moon has any atmosphere at all; but most assuredly if she have any it must be very limited in extent. When the moon passes over a star, the disappearance of the star is quite sudden; there is no sign whatever of that gradual diminution of the star's light which would undoubtedly be recognised if the moon had an atmosphere of appreciable extent.

The French astronomers, La Hire and De Lisle, put forward two theories, which are now obviously untenable. According to each theory, the appearance of the corona is caused by an action on the sun's rays, that action taking place at the edge of the moon's disc—the difference between the two theories being that La Hire ascribed the action to the inequalities of the moon's surface and their power of reflecting the solar rays, while De Lisle supposed that the sun's rays were diffracted at the moon's edge. We owe to Baden Powell and Sir David Brewster the disproof of De Lisle's theory, De Lisle himself having disposed of La Hire's.

There remains, then, only one theory to consider, the theory, viz., that the corona is a true solar appendage, and one of the most remarkable features in the universe (for the other, that the corona is simply a terrestrial phenomenon, due to the passage of the sun's rays through our own atmosphere, has never been supported by anyone competent to express an opinion on such matters, and has now disappeared even from the writings of those who advanced it).

The corona cannot be a solar atmosphere. It will be obvious that if the corona were such an atmosphere, it would exert a pressure upon the sun's surface corresponding to that pressure which our own atmosphere exerts upon the surface of the earth. But then the pressure exerted by the coronal atmosphere would be incalculably greater. Our own atmosphere, we have reason to believe, does not extend much more than 100 miles above the sea-level. Now the corona is visible, under favourable circumstances, at a distance from the sun equal to his own diameter—setting aside all considerations of the radial projections. In other words, it certainly does not extend less than 850,000 miles from his surface. Regarded as an atmosphere, therefore, the corona is certainly not less than 8,000 times as deep as our own. On this account alone the pressure it would exert would be enormously greater. For it is to be noted that the pressure exerted by our air would not be merely doubled were the height of the atmosphere doubled, trebled were that height trebled, and so on, but would increase at a much more rapid rate. If a mine were sunk into the earth in order to measure the increase of atmospheric pressure with depth, instead of a depth of 100 miles being required in order to have a double pressure, only 3½ miles would be needed. At the bottom of a mine 7 miles deep the pressure would be four times as great as at the sea-level; 10½ miles deep the pressure would be eight times as great; 14 miles deep the pressure would be sixteen times as great, and so on, like the expense of the miser's grave, 'doubling as we descend' for every 3½ miles. It requires no great knowledge of arithmetic to see that the pressure at a depth of 100 miles or so would be millions of times greater than that at the sea-level.[1] It will be seen, therefore, how inconceivably great the pressure exerted by a solar atmosphere some 8,000 times as deep as ours would necessarily be, let the nature of the gases composing it be what it may.

But even this is not all. We have hitherto only compared the height of the supposed solar atmosphere with that of the earth's. We must not forget that the sun's attractive energy so enormously exceeds the earth's that even though his atmosphere were no deeper than ours (and similarly constituted) the pressure exerted on his surface would be enormously increased. If a man could be placed on the solar surface his own weight would crush him as effectually as though while on the earth a weight of a couple of tons were heaped upon him. In precisely the same way the pressure of the solar atmosphere is increased by the enormous force with which the sun drags towards himself every particle composing that atmosphere.

Now it happens that we know quite well that the pressure exerted by the real solar atmosphere, even close by the bright surface which forms the visible globe of the sun, is nothing like so great as it would be if the corona formed part of that atmosphere. The bright lines constituting the spectrum of the coloured prominences would be many times thicker than they are if the pressure were so great; for spectroscopists have found, by means of experiments made in the laboratory, that with increase of pressure the spectral bright lines of a gas increase in thickness.

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[1] The actual number representing the proportionate pressure would consist of no less than nine figures, being very nearly two hundred millions.

Here, then, we have the most conclusive proof possible that the corona is not a solar atmosphere.

But, on the other hand, those who argue that the corona is a solar appendage, ask how it happens, if the phenomenon is due to the illumination of our own atmosphere, that the moon looks black in the very heart of this illumination. If our air were illuminated, its light would extend over the moon also—since the moon lies so far beyond its limits; whereas the moon is as a dark disc on the background of the coronal light. This very word background, obviously applicable to the corona as actually seen, indicates that the source of the coronal light is beyond the moon.

Here, then (to mention no other considerations), we have the most conclusive evidence that the corona is not a phenomenon of our own atmosphere.

But then the corona is clearly *somewhere* and *something.* If its light comes from beyond the moon, we need not doubt that it comes from the sun's neighbourhood; and again, if the corona is not a solar atmosphere, we can scarcely doubt that it is a solar appendage. It would seem to follow that the corona is due to bodies of some sort travelling around the sun, and by their motion preserved either from falling towards him (in which case the corona would quickly disappear) or from producing any pressure upon his surface, as an atmosphere would.

Whatever the corona may be, it is clear that regarding it as a solar appendage—a conclusion which seems forced upon us by the evidence—it is presented to us as one of the most striking and imposing of all the phenomena of the solar system. It is a fitting crown of glory for that orb which sways the planets by its attraction, warms them by its fires, illuminates them by the splendour of its light, and pours forth on all of them the electric and chemic influences which are as necessary as light and heat for the welfare of their inhabitants.

THE SUN'S LONG STREAMERS.

Professor Cleveland Abbe, an American astronomer and meteorologist, who had intended to observe the eclipse of the sun in July 1878 from the summit of Pike's Peak, in Colorado, more than 14,000 feet above the sea-level, fell ill after he had reached that place, and was carried down to the Lake House (elevation 10,000 feet), there to remain while the rest of his party stayed to view the eclipse from the summit. Probably if he had remained with them his observations would have differed in no very marked degree from those which other astronomers made on that occasion. He would have devoted a few seconds, perhaps, to the study of the sun's corona with the naked eye. He would probably have made some telescopic, spectroscopic, or polariscopic observations during the rest of the three minutes during which the total eclipse lasted, and possibly he might have noted some feature rather more effectively and satisfactorily than most of the other observers. But under the actual circumstances he could not hope thus to take his place among the thousands of observers who have noted the phenomena of total solar eclipses. He had no optical or other instrument. Worse than all, he is near-sighted; and though he had a pair of spectacles, it was not quite strong enough to correct his near-sightedness.

Yet Professor Abbe succeeded in making observations far exceeding in interest any which were made by the entire force of eclipse observers in 1874 and 1875, and fairly comparable in this respect with the most remarkable discoveries effected during the great eclipses of 1868, 1869, 1870, and 1871. Debarred from instrumental researches, unable to do what most observers of eclipses seem anxious to do—namely, to see everything that can be seen—he was compelled to restrict himself to precisely that line of observation which we indicated eight years ago as likely to be most instructive. He gave his whole attention to the corona, and especially to its outlying and feebler portions. Studying the phenomena with the naked eye, or at least with only spectacles to aid him, he could recognise faint luminosity which the telescope would inevitably have concealed from his view. He was not hurried; nor was he disturbed by the thought that such and such instruments must be attended to in turn while still totality lasted, with care also that in the darkness nothing should be disturbed or injured. As he said after the observations were completed, and as I pointed out in 1870, 'a glance of a few seconds will no more suffice to do justice to the delicate phenomena [of the corona] than it would suffice to enable a naturalist to draw the distinguishing features of a new shell or insect, or would enable an artist to correctly sketch in a landscape.'

Before describing what Professor Abbe actually saw, it may be well to indicate first the nature of the observations he proposed to make, and secondly his preconceived ideas as to what he was likely to see, for otherwise the value of his observations will not be fully appreciated.

My readers may perhaps remember that in the year 1870 a discussion took place on the question whether the glory of light seen around the sun during total eclipse belongs to the sun or not. There were those who maintained very confidently the opinion that this glory is either a purely optical phenomenon only or else is due to the passage of the solar rays through our own atmosphere all round the place of the eclipsed sun. On the other hand, there were some (myself among the number) who pointed out that the corona must necessarily belong to the sun, since its features could not possibly be reconciled with any other theory. The greater number of astronomers seemed, however, to form no opinion one way or the other, but to prefer to leave the matter to be decided by fresh evidence. For too many imagine that the best way of showing how greatly they value observations is by declining to investigate the full significance of observations already made.

It will be remembered that before long the new observations devised to settle a question which had been abundantly answered by observations already made proved unmistakably the solar nature of the corona. Photographs were taken during the total eclipse of December 1870, and in greater number during that of December 1871. On the latter occasion photographic views of the corona taken at stations far apart agreed closely together, showing that the corona could not possibly be an atmospheric phenomenon. No one could imagine that the air above Baicull, where Mr. Davis (Lord Lindsay's photographer) took his views, could by some amazing accident produce coronal features resembling those produced by the air above Ootacamund, one station being close to the sea-shore, the other hundreds of miles inland and some 10,000 feet above the sea-level. On the other hand, the resemblance of the several views taken at either station showed that the coronal glory could not be due to the illumination of some matter on the hither side of the moon, but far outside our own atmosphere. For the solar rays, passing athwart the lunar disc to fall upon such matter, would shift rapidly in position as the moon moved onwards, so that the features seen at the beginning of total eclipse would differ markedly from those seen towards the end. Since the six pictures taken at Baicull closely resembled each other, as did the six taken at Ootacamund, so that all twelve views represented the same corona (though of course not all to the same distance from the sun), it was manifest that the corona then seen was a solar appendage. The actual distance to which the corona can be traced in these pictures corresponds to about 900,000 miles.

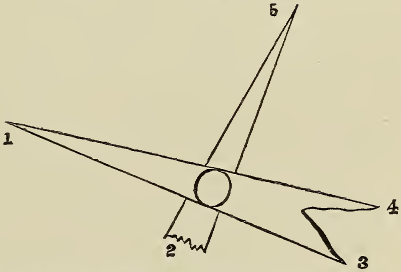
But the believers in an atmospheric corona were not even yet wholly satisfied. Nay, before the recent total eclipse one among them even went so far as to say that the observations and photographs of 1870 and 1871, while demonstrating the solar nature of the glory immediately surrounding the sun, proved the long rays extending much farther from the sun to be non-solar phenomena. 'The non-solar origin of the radial structure,' said Mr. Lockyer as late as July 20 last, 'was conclusively established' during the eclipse of December 1871.

To say the truth, there is no possible way of interpreting the long rays as phenomena of our own atmosphere or of matter (gaseous, meteoric, or dust-like) on the hither side of the moon. The idea is one which mathematicians may casually have thrown out. Indeed, Mädler and Airy, after the eclipse of 1860, advanced the hypothesis that the long rays belong to matter between us and the moon, while Sir John Herschel adopted in his 'Familiar Lectures' the notion that these rays belong to matter at a great height in our own atmosphere. But it would be to misrepresent these eminent astronomers to assert that they ever *maintained* these views. The available evidence, analysed as any one of these mathematicians could have analysed it, had he seen fit, would have shown convincingly that the rays must come from matter lying far beyond the moon. Sir John Herschel admitted this in a letter addressed to myself. Whether Airy or Mädler ever examined the evidence closely I do not know. If they did they doubtless were led to the same result as Sir J. Herschel. The matter may be put in this way:—Since these long rays extend from the black disc of the moon during mid-totality, they occupy then a part of the sky where no sun-illuminated air lies at such a time; therefore they cannot belong to our air: but if there were some very tenuous matter, aërial or dust-like, extending as far as the moon's orbit, the whole region of the sky athwart which these rays extend would contain matter of this sort under full solar illumination: no rays then would be seen, but a nearly uniform glare, which should become brighter and brighter as the distance from the sun's place increased. If we add to this that at midnight the whole of the sky, except a round spot some four or five times the diameter of the moon, would be occupied by this cis-lunar matter under direct solar illumination, instead of that illumination from behind which such matter would receive during total eclipse, we see that the darkness of our midnight sky speaks as decisively against this theory as does the brightness of the long rays seen during total eclipse.

Notwithstanding the overwhelming evidence available to show that these rays lie far beyond the moon, Professor Abbe had adopted the opinion that the rays belong to the earth's atmosphere, or else are mere optical illusions. 'I had hitherto firmly believed them,' he says, 'to be either in the earth's atmosphere or in the observer's eyes.' 'Such rays,' he adds, 'were seen by members of my eclipse party at Sioux Falls City, Dakota, August 1869; but at that time and ever since I have doubted their existence.' It is manifest that he did not begin his observations with the preconceived idea that the rays belong to matter far more distant than the moon, but with a strong opinion, if not a strong prejudice, the other way.

Next let us consider the actual circumstances under which he observed the eclipse, for they also are important in enabling us to estimate the value of his result. 'Having been somewhat hastily carried,' he says, 'from the summit of Pike's Peak down to the Lake House (elevation 10,000 feet), I had by Monday noon recovered sufficiently to be laid on the ground upon a gentle slope facing westwards, where I studied the rays visible about the sun during totality. I had no optical or other instrument, and unfortunately had only a pair of spectacles not quite sufficient even to correct my near-sightedness. By straining my eyes somewhat I was, however, able to do something. My whole attention was given to the rays that extended beyond the brilliant ring which I presume represents the true solar atmosphere. I was undisturbed by any other consideration except to get a true presentation of these rays. . . . I went over the region around the sun again and again—at least six times—leisurely during the 161 seconds of totality, and cannot doubt the truthfulness and fairness of my drawing and description. . . . Two stakes were driven down on either side of me ; and between them was placed a rotable axis, on which my drawing-board and paper were fastened. . . . By slightly tipping my drawing-board I kept the sun just above it, or just hidden from view, as I wished, while I drew in such details as I wished, and that too, as it seemed to me at the time, with great ease and accuracy, especially as to the angular position of the rays.'

The moon or sun appeared surrounded by a narrow brilliant white ring, less than 140,000 miles broad. (We alter the technical indication of apparent breadth into the actual breadth in miles as likely to be more intelligible to most of our readers.) This ring was as brilliant as the full moon. It was of uniform tint and light, continuous and without any break or structure visible to Professor Abbe. 'Outside of this there was no other concentric coronal appearance and no external boundary ; but the immaculate blue black sky immediately adjoined this light, which I now call the true solar corona or atmosphere.' There was throughout plenty of light to read and write by, though very different from that given by the full moon.



Illustrating the rays seen round the eclipsed sun by Professor Abbe.

The picture which accompanies Professor Abbe's description in the *Colorado Spring Daily Gazette* is doubtless not intended to present with any accuracy the actual tints or degrees of brightness of the various features observed. The shape of the streamers is shown with sufficient exactness in the accompanying figure. It will be understood, of course, that the rays numbered were seen on a dark background, the 'immaculate blue' of Abbe's description.

The tapering ray marked No. 1 was the first seen by him. He says he saw it on his first glance at the corona. It then seemed to extend about three times the diameter of the sun; but in a minute or so, as the observer's eyes became accustomed to the sight, he was able to trace its tapering end to a distance of six diameters of the sun's disc. 'Its sides were straight lines, its axis passing slightly below the sun's centre. Its light was an exceedingly faint and delicate white, apparently overlaid or intermingled with the blue of the atmosphere, I saw no striation, texture, or variation of light. There was no decided increase of brightness in that part of the ray near the sun's edge, nor in the axis of the beam, the delicate light continuing uniform up to the corona, in whose glare it was lost.' We must note here two points. In all probability the words 'in a minute or so' are used in their colloquial sense for *presently*, because the whole totality did not last two minutes and half, and in the course of that time Professor Abbe noted all the features of the corona six several times. Secondly, we find that both in the *Daily News* and in *Nature* Professor Abbe is described as tracing the rays to a distance of six degrees from the eclipsed sun, not six diameters only; so that, as the sun's apparent diameter is little more than half a degree, these accounts would suggest that he saw the rays to double the distance described in the *Colorado Daily Gazette.* But there seems little reason to doubt that the accounts given in the *Daily News* and *Nature*, which constitute in reality but one account, seeing that they both came from the same source, are incorrect; for the account sent to the Colorado paper was written by Professor Abbe himself. It contains an illustration from a drawing of his own (reproduced above), which agrees with his description. Moreover, we received the paper directly from Professor Abbe; and unquestionably he would have struck out the word 'diameters' and substituted 'degrees' if he had really seen the ray extending to the greater distance. Note also that the word 'diameter' is used throughout the descriptions of other rays.

The ray marked 2 was seen as soon as 1. Its bounding edges, diverging from each other, but not from the sun's centre, produced a somewhat fan-shaped ray. 'When first seen,' says Abbe, 'I estimated its outer limit at one diameter, but subsequently traced it to a diameter and a half from the sun. Its left-hand edge appeared somewhat sharper and brighter than the right-hand edge. With this exception the light was very uniformly distributed throughout its surface, fading away rapidly at its outer end. It also remained changeless throughout the totality.'

No. 3 was also seen at the same time as No. 1. 'It was narrower and shorter than No. 1: its estimated length, three diameters. It broadened at its base, like No. 1, and had the same uniform tint and intensity.'

No. 4 'was not noticed at all until the totality was half over. Its length was one diameter, and it was certainly brighter at the end farthest from the sun. It remained perfectly steady,' adds Professor Abbe, 'after I once noticed it, and gradually I became aware of a faint light partially connecting it with No. 3, so that the final impression left on me was that these two constituted one fan-shaped projection similar to No. 2, but fading out in the central portions. The axis of No. 1 and of Nos. 3 and 4 passed nearly, if not exactly, through the sun's centre.'

No. 5 extended fully five diameters from the sun's limb, 'and was in all respects similar to No. 1. Its base was broader than that of No. 1, which I attributed,' says Abbe, 'to the glare of the increasing corona' and of a mound of the ruddy prominence matter (low-lying, so as to form only an extension of the sierra). The light of No. 5 was fainter, Professor Abbe thought, than that of No. 1. 'Its edges were straight, except in so far as the coronal glare appeared to unduly broaden the base. Its axis passed very nearly through the sun's centre, and was in the prolongation of the axis of No. 2.'

Professor Abbe's explanation of these rays or streamers occurred to him an hour or so after seeing them. He advances it as one which 'will probably result in the overthrow of all previously entertained theories respecting the character and cause of these streams of light.' But in reality it is not nearly so novel as he seems to imagine. It is, indeed, partly new, and in my opinion it is in great part true; but what is true in it is not new, and I question greatly whether what is new in it can possibly be true. Let astronomers judge.

'Meteor streams,' says Professor Abbe, 'is the key to the solution—not such meteors as some suppose to be falling into the sun daily, but the grand streams of meteors that cause the numerous shooting stars of August and November, and of the existence of which there is indubitable proof. These streams consist of fine particles or pieces, each a long way from its neighbour, but all rushing along in parallel orbits, about the sun, like the falling drops of rain in a thunder-shower. The August stream is calculated to be several hundred thousand miles broad and thick, and many million miles long. Such a stream, when far beyond the sun, but still lighted up by it, would reflect to us a faint uniform light precisely like that of these rays. If one end of the stream were farther from us than the other, the effect of the perspective would be to produce a tapering or wedge-shaped appearance. In some other part of our orbit, or with the meteor stream in some other part of its orbit, the perspective might vanish and the two ends appear of the same width. In this way we shall undoubtedly be able to explain the very numerous historical and memorable occasions on which flaming coronas, swords, comets, &c., seen in the sky during a total eclipse have been regarded by the superstitious as Divine omens.'

I have very little doubt that the great extension of the corona in certain directions during many total eclipses, and the probably far greater extension of a fainter, not readily discerned lustre during all eclipses, is due to the existence of meteor streams. It is also undoubtedly true that several of the meteor systems encountered by our earth in her journey round the sun have the vast dimensions mentioned by Professor Abbe. Indeed, he far underrates the dimensions of the August and November meteor systems, each of which must be measured in length by hundreds of millions of miles, not by mere millions. But it is absolutely impossible that any of the meteor systems traversed by our earth, or any meteor systems of no greater degree of richness, should present the appearance of streamers surrounding the sun like those in our figure above. So far as the two systems specially mentioned by Professor Abbe are concerned, inasmuch as we know the exact shape and position of the orbits along which the meteors forming these systems travel, we can determine the exact position which the meteoric streams occupy in the heavens at any moment; and most certainly neither of them on July 29 last occupied the position of the two beams shown across the sun in our figure. The August system was the one which at the time passed nearest to the sun's place on the sky, but it did not come within several degrees of the sun. The November system did not even cross the part of the sky where the sun was. These two systems, therefore, could not possibly be connected in any way with the two streams, of whatever nature, which produced the rays intersecting exactly at the sun.

But there is a more general objection to the theory that such meteor systems may explain coronal streamers seen during total eclipses of the sun. If such streams could be seen when situated beyond the sun, they would be seen far better when opposite the sun on the dark background of the midnight sky. Take, for instance, the November meteors. We know that the flight of meteors, some 2,000 millions of miles long, which the earth traversed in November 1866, 1867, 1868, 1869, 1870, and 1871, is now nearing the remotest part of the long orbit of the November system, many millions of miles beyond the path of Uranus. We know that at midnight in winter the richest part of that system lies due south, at an elevation varying from thirty to fifty degrees above the horizon. There, illuminated fully by the sun, though at a great distance from him, it ought to be far better seen than a similar system lying beyond the sun and visible only through the light of the brightest part of the corona. But no one has ever, on the darkest and clearest night and under the most favourable atmospheric conditions, even suspected the existence of the faintest possible light where the heart of the November system is really situated. Much less, then, could such a system be seen during total eclipse (if so situated as to lie athwart the sun). Systems less rich than the November system (the richest known to us) would have still less chance of being discerned.

If, then, we are to account for the radial streamers seen by Professor Abbe, and also seen during many other total eclipses, though to a less distance, by the meteoric theory, we must consider meteor systems very unlike those through which the earth herself passes. The meteor systems required by the theory must be much denser and much more brightly illuminated than the August and November systems. To say they must be much more brightly illuminated is equivalent to saying that they must be much nearer the sun. And in this we see an escape from another difficulty. Meteor systems very near the sun would be far more likely to appear as streamers extending radially from him than systems at a great distance from him. A distant system might, by a mere chance, so appear. For instance, if a total eclipse of the sun had occurred on or about May 10, 1865, the November meteor-system (whose richest part was then crossing the earth's track at the point she occupies on November 13) would have appeared, if discernible at all, as a streak athwart the sun's place in the sky, and therefore forming two rays on opposite sides of him, somewhat like 2 and 5 in our figure. Sixteen years or so earlier or later the November system would present a similar appearance, only very much fainter, on account of greatly increased distance, during a total eclipse occurring on or about November 13. At no other time in the year except November 13 and May 10, or about these dates, could the November system present such an appearance. But a system travelling close to the sun, and not far from the plane near which all the planets travel, would present at all times nearly the appearance of a pair of rays like 2 and 5 of our figure. On this account, therefore, as well as on account of the greater brightness with which such meteor systems would be illuminated, we must prefer the theory that the systems to which the coronal rays are due travel near to the sun.

Yet, even as thus presented, the meteor theory alone seems inadequate to explain the coronal streamers. There is an enormous mass of evidence showing that meteor systems are most richly strewn throughout a region around the sun extending nearly to the distance of the planet Mercury; but there is also abundant reason for believing that these multitudinous systems would present an appearance very different from that depicted in Professor Abbe's view of the coronal streamers. We want something quite distinct from the theory of a mere aggregation of meteors to account for these rays, whether pointed or fan-shaped, extending directly from the sun. The aggregation of meteors might present the appearance of a luminous cloud around the place of the eclipsed sun. This cloud might be to some degree radiated, because each meteor system would have a course carrying it either directly athwart the sun's place on the sky, or nearly so. But there would be nothing like those sharply-defined streamers extending separately from the sun to distances of ten or twelve sun-breadths. Sir George Airy, describing the appearance of the corona during the eclipse of 1851, pictures just such a cloud as we should expect to result from the aggregation of meteors.

'Its colour,' he said, 'was white, or resembling that of Venus; there was no flickering or unsteadiness; it was not separated from the moon, nor had it any annular structure: it looked like a radiated luminous cloud behind the moon. The long streamers manifestly require a different explanation.

I cannot but think that the true explanation of these streamers, whatever it may be (I am not in the least prepared to say what it is), will be found whensoever astronomers have found an explanation of comets' tails. These singular appendages, like the streamers seen by Professor Abbe, extend directly from the sun, as if he exerted some repellent action on the matter forming the heads of comets. Indeed, Sir John Herschel did not hesitate to say that the existence of such a repulsive force was, to all intents and purposes, demonstrated by the phenomena of comets' tails. Now we know that meteors and comets are in some way associated, though the actual nature of the connection between them is not clear. It is certain that the November meteors, the August meteors, and other such systems, follow in the track of known comets. We know that when, in 1862, the earth passed through the region of space along which Biela's comet had recently travelled, there was a display of thousands of meteors, all radiating from just that part of the heavens from which bodies travelling parallel to the orbit of Biela's comet would have seemed to radiate. It follows from this association between comets and meteors, and from the fact that probably thousands of meteoric and cometic systems travel close to the sun, that in all probability there must exist generally, if not always, in the sun's neighbourhood, enormous quantities of the substance whence comets' tails are formed by the sun's repellent action. This being so, we should expect to find generally, if not always, long streams of matter extending from the sun's immediate neighbourhood, in the same way that comets' tails extend from comets' heads. Whether the repulsive force is electrical, magnetic, or otherwise, does not at present concern us, or rather it does concern us, but at present we are quite unable to answer the question. All that we know certainly is that, in the first place, the sun does in some way cause streams of luminous matter to appear beyond the heads of comets, in a direction opposite to his own, and to enormous distances ; and, in the second place, that the matter forming comets' heads is probably present at all times, in large quantities, in the sun's immediate neighbourhood. We can hence infer, with extreme probability, that such long streamers as Abbe saw last July, Myer in August 1869, Feilitsch in June 1860, and several Swedish observers during the eclipse of 1733, are produced in the same way as comets' tails, and therefore really extend (as they seem to do) radially from the sun. It is also certain that if they did not extend radially from the sun, their always seeming to do so would be altogether inexplicable. So that the theory to which we are led in one direction leads us also out of what would else be a very perplexing difficulty in another direction.

Some recent inquiries which I have made have led me to the belief that the radial streamers are after all meteor streams, opposite streamers (in the cases cited above) being parts of the same meteor streams lying (at the time) beyond the sun, so as to be seen athwart his disc.

METEORIC ASTRONOMY.

THE views respecting meteors now held by astronomers are of such extreme importance, whether viewed directly, or regarded in relation to the inferences which seem to flow from them, that they may be regarded as affecting our ideas respecting the present constitution as well as the past history and the future fate of all the orbs which people space. I propose to consider the position to which meteoric astronomy has at present been brought, and to point out the connection between the results now established, and the subject of the solar corona, which has recently occupied so large a share of the attention of astronomers.

We need not consider here the history of the earlier meteoric theories. It would be difficult to show that the mere correct ideas of the Greek and some of the Roman writers who have spoken of meteors were less purely speculative than the later view that meteors are mere phenomena of our own air, like lightning or the aurora. And although the theory that meteors are bodies which have been expelled from lunar or planetary volcanoes was discussed by mathematicians of eminence, yet it was not based on exact observation; so that the calculations of Laplace, Obbers, and others, serve rather to illustrate the skill of those mathematicians than to establish any conclusions of value.

We may begin, then, by considering the first important fact tending to prove the extra-terrestrial nature of meteors and shooting-stars—the circumstance, namely, that meteoric displays occur commonly on certain days of the year.

It had been noticed in very early times that a display of shooting-stars nearly always occurs on the night of August 10. This being known in calendars as St. Lawrence's Day, the meteors which fall on that day have been called the tears of St. Lawrence. Among astronomers, however, they are more commonly called Perseides, for a reason presently to be cited. Not so early, but still many years before the true theory of meteors began to be recognised, it was known that on or about the 12th or 13th of November shooting stars are commonly seen. When Humboldt, after witnessing the remarkable display of 1799, invited special attention to this circumstance, ancient records were examined, and it was found that for several centuries this particular part of the year had been characterised by star-showers. 'Time out of mind,' says Sir John Herschel, 'those identical nights more often, but sometimes those immediately adjacent, have been habitually signalised by such exhibitions.'

It cannot be too often insisted upon—since doubts are frequently expressed respecting the truth of the modern theory of meteors—that this circumstance of periodicity suffices of itself to demonstrate the extra-terrestrial nature of these objects. There are no meteorological phenomena which recur persistently on August 10 and on November 13; terrestrial volcanoes are not *then* exceptionally active; the moon on those dates may be in any part whatever of her orbit. The one circumstance to which phenomena recurring on a particular date can be held to point is the recurrent passage by the earth of a particular part of her orbit on that date. If we picture the earth circling around the sun in her wide orbit, once in each year, and remember that year after year as she crosses the particular point corresponding to August 10, and again as she crosses the particular point corresponding to November 13, her air is alive (as it were) with meteors, we at once see that this is because she comes across the meteors at those stages of her circuit. It is precisely as though a person who travelled continually on a certain road noticed always at certain stages some peculiarities, such as heat or damp, or the like. He would be certain, after a few experiences of the sort, that the phenomenon was local in its nature—peculiar, in fact, to the particular part of the road where it was recognised. So we, who voyage with the earth on a wide path round the sun, must conclude—must be absolutely certain—that the two particular regions of circumsolar space which we traverse on August 10 and November 13 are swarming with meteors.

Yet we cannot for a moment imagine that two clouds of meteors are persistently present in these two regions. Each meteor is as surely acted upon by the sun's mighty influence as this earth on which we live; and as surely as this earth, if brought to rest in any way, would be attracted towards the sun and fall upon his globe in about 64½ days, so every member of a meteor-cloud placed where the August and November meteors are encountered would in about the same time fall upon the sun and be destroyed.

It follows that the meteors must be in rapid motion, on a course keeping them clear of the sun's orb; and moreover, that the place of those which pass away from the region traversed by the earth must be more or less continuously supplied by arriving meteors. In other words, the August and November meteors must form a more or less complete zone or ring. The degree of completeness of either ring must correspond to the regularity of the recurrence of star-falls on the dates corresponding to either system. If it frequently chances that the display is intermitted, either for a few years or for many years in succession, the inference will be that greater or less gaps mar the completeness of the meteor zone; whereas, if no year passes without a display of meteors belonging to a system, we must infer as at least probable that the meteor system forms a complete ring. Thus judged, the November system appears to be very far from forming a continuous zone; since the display is often omitted for more than twenty years in succession, and is seldom repeated during more than four or five successive years. The August system, on the contrary, seldom fails to produce a display of greater or less splendour.

So much may be regarded as demonstrated by the evidence already referred to. But to remove any doubt which may remain as to the justice of the inference that the August and November meteors are bodies travelling in a definite region of interplanetary space, there remains a test of a decisive nature.

Since according to the hypothesis, all the August meteors are travelling around the same orbital zone, they must cross the earth's track on paths appreciably parallel, moving also with equal velocities. Now, regarding the earth with reference only to her orbital motion, we see that every meteor of the August system which actually encounters the earth must reach the earth's globe from one and the same direction in space. If the earth were actually at rest, the meteors of this system would seem to fall like a shower coming from a definite quarter. This, indeed, might not appear to be the case to observers occupying a particular point on the earth's surface, simply because the meteor-shower would be only partially discernible by him, and the circumstances would be such as to cause some illusion as to its origin and nature. But if we regard the earth as a whole—and for a moment as a sentient being—it will be obvious that, supposing she were at rest, she would seem to be exposed to a meteoric shower, many meteors falling upon her and yet larger numbers passing by her, but all appearing to come from the same quarter. But taking her orbital motion into account, we obtain a precisely similar result, only the direction of the shower becomes modified. The case may be compared to that of a railway carriage in a steady shower of rain ; such a shower will appear steadily to proceed from one quarter of the sky whether the carriage is at rest or moving uniformly in a given direction. *Only* in the latter case, the part of the sky from which the shower seems to proceed will seem to be somewhat nearer to the point of the horizon towards which the train's motion is carrying the carriage. So with the earth passing through a meteor stream; the meteor-shower will seem to fall from one and the same part of the celestial sphere though the earth is travelling rapidly onwards; *only* the part of the heavens from which the shower seems to proceed will seem to be somewhat nearer to the point towards which the earth's orbital motion is carrying her than in the imaginary case of a fixed earth exposed to the continuous downfall of the meteors belonging to the August system.[1] But in either case the shower will seem to come

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[1] In considering this matter we need not concern ourselves with questions of perspective, which, though commonly introduced to explain the subject, tend rather to perplex than to enlighten the learner. The case is exceedingly simple in reality :—

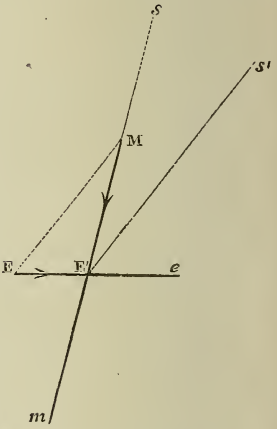

FIG. 1.—Illustrating the existence of meteoric 'radiant points.'

Thus, let E *e*, Fig. 1, be a small part of the earth's orbit at the place where any meteor system is encountered, and let M *m* indicate a portion of the meteor-orbit, M E′ being the course travelled by a particular meteor while the earth is moving along E E′, so that this meteor encounters the earth when she has arrived at E′. Then to the observer on earth unconscious of the motion from E to E′, the course on which the meteor seems to arrive is obviously in the *direction* of a line from M to E, so that a line, *s*′ E′, parallel to M E is the seeming path of the meteor's arrival; and a point *s*′ on the celestial sphere, in the direction E′ M′ indefinitely produced, is the point of the heavens from which that meteor seems to proceed. But the directions E′ E′ and M E′ are constant and the proportion of E E′ to M E′ is constant; therefore the direction E M or E′ M′ is also constant. Hence, every meteor of the system must appear to proceed from the point *s*′ on the celestial sphere.

In reality every meteor of the system travels parallel to M *m*, and therefore is proceeding (at the time of crossing the earth's track) as if from a point *s* on the celestial sphere in the direction E′ M indefinitely produced.

Since the visible part of a meteor's course in our atmosphere is a from a determinate point of the star-sphere enclosing the earth on all sides.

Similar remarks apply to the November shower.

Thus a test of the interplanetary nature of meteoric systems is at once afforded, and a test of a remarkably effective nature. For owing to the earth's rotation the star-sphere appears to rotate ; and thus the part of the heavens whence a meteor shower seems to proceed is continually changing its place with respect to the horizon. *If*, notwithstanding this continual change of apparent place, a certain region of the stellar heavens continue to be that whence a meteor shower seems to come, the evidence is so much the stronger, because the test is applied under continually varying conditions.

In the case of both the meteor systems referred to, the test confirms the result already deduced. The meteors of each system seem to come from a definite region of the heavens, or rather from a definite point of the star-sphere. In the case of the August meteors this point lies towards the north-eastern part of the constellation Perseus, or, more exactly, the R.A. of the point is about 3 h. 24 m., its north declination about 55°. The November meteors appear to come from a point in the constellation Leo, between the stars ϵ and ζ of that constellation, or, more exactly, the R.A. of the point is about 9 h. 52 m., its north declination about 24°. These points are called the 'radiant points' of the meteor systems, and because of the position of those points the August meteors are commonly called the Perseides, while the November meteors are called the Leonides.

It would be necessary to enter at this point on a consideration of the appearances actually presented by the August and November shooting-stars during any consider-

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straight line (approximately) directed from one and the same point of the heavens (so far as a given meteor system is concerned), it follows that wherever the observer may be, that course must appear to be such that if produced backwards it would pass through that part of the heavens.

able display, if it were my present purpose to give a full account of what is known respecting meteors; but I wish, at present, to restrict the reader's attention as far as possible to the cosmical aspect of the subject.

The test here considered becomes, after it has served this particular turn, the means of proving the existence of other meteor systems which produce no recurrent showers, or no displays so persistently recurrent as to leave us assured of their being due to a true meteor system.

For, if many meteors seen on any night appear to radiate from a particular part of the heavens, we can infer very probably, though not quite certainly, that those meteors form a single group travelling all in the same direction. We can infer this, because we know that the observed event would certainly happen if the meteors form such a group; but we cannot be quite certain that one group only is in question, because two meteor groups crossing the earth's track at the same place might have different directions and velocities so adjusted that the members of each would seem to encounter the moving earth in the same direction. The unlikelihood of the coincidence renders the inference that a single group is in question so much the more probable. Now if, on the same night of other years, or very nearly at the same date, more meteors are seen to proceed from the same radiant point, we conclude that not a group but a system is in question, since at the very spot where meteors were really travelling in a definite direction in one year, other meteors are found following in their track in later years. Many millions of miles must separate the first set from those seen later; and the inference is, that along the whole range of those millions of miles there are meteors travelling with equal velocity in one and the same direction —in other words, traversing a definite orbit region.

Applying this principle, the observers of meteors—Alexander Herschel, in England; Professor Newton, in America; Secchi, in Italy; Heis, in Germany; Quetelet, in Belgium; and Schmidt, in Greece; besides many others—have established the existence of nearly 200 distinct meteor systems having radiant points north of the equator; while Neumayer, at Melbourne, and other observers, have noted many others having southern radiant points. It may be concluded safely that upwards of 300 meteor systems cross the earth's orbital track around the sun. Counting—as resulting probably from the existence of corresponding but more important systems—those falls of fireballs and of aërolites which have been observed to occur on or near certain definite days, and taking also into account the probability that some meteoric systems have hitherto escaped recognition, we may infer that some 400 systems of bodies traversing the interplanetary spaces pass close to the earth's orbit.

It does not follow, however, be it noted, that any one of these systems centrally crosses the earth's path, whether we regard that path as the elliptic line traversed by the earth's centre, or as the elliptic ring traversed by the earth's globe. On the contrary, the chances are that the earth does not pass quite or even nearly through the *core* of any meteor system belonging to the solar domain.

Now when Schiaparelli commenced his inquiries, absolutely nothing was known about the extent of any one of the meteor systems traversed by the earth. To take the August and the November systems, it was known that one part of each lies near the earth's orbit. It was known also that the August system crosses the earth's track at a considerable angle, since even though the earth's motion brings the radiant point down (as it were) nearer to the point towards which the earth is making on August 10 (a point close by the star δ Arietis), the radiant is still some 37° from the ecliptic. It was known further that the November system travels much nearer to the plane of the ecliptic, and in a retrograde course (that is, meeting the earth), because the radiant lies within 10° of the ecliptic, and almost in the latitude of the point towards which the earth is travelling on November 13 (a point close by ψ Leonis). But nothing whatever was known as to the orbit range of either system—

the major axis of the mean path of these meteoric families. For anything that had been shown to the contrary (setting aside as too unsatisfactory to be trusted the estimates of the velocities of the meteors while traversing our atmosphere), the part of the systems traversed by the earth might be the portion farthest from the sun, and the perihelion portion might be quite close to his orb ; or the part traversed might be the perihelion portion, and the aphelion portion might have any distance whatever—from a range little exceeding the earth's mean distance to one exceeding many times even the distance of Uranus or Neptune. This ignorance as to the meteoric orbit-ranges was necessarily accompanied by ignorance as to the real velocity with which they cross the earth's track, and this in its turn involved ignorance as to the true inclination of either system. Given the major axis of either meteor orbit, the real velocity of the meteors at the earth's distance would be known ; and since the earth's motion is known in velocity and direction, the apparent direction of the meteor's motion being also known, the real direction of the meteoric motions follows at once. That this is so can readily be demonstrated. Thus, let E E′ be taken to represent the earth's velocity in magnitude and direction, and let *m m′* represent in magnitude the real velocity of any meteors which seem to come in the direction *s′* E; then we have only to describe the circular arc, K L M, around E′, with *m m′* as a radius, to obtain E′ M, the real direction of the meteor's motion. (Compare the note illustrated by Fig. 1.) But this very construction shows us that so long as the real meteoric velocity, *m m′*, is unknown, we cannot tell the real direction, *s′* E, of the meteor's motion. The apparent velocity, M E, would indeed give us what we want (the point M, in fact) ; but a meteor appears too suddenly and vanishes

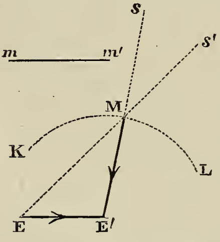

FIG. 2.—Illustrating the determination of the real course of meteors when their 'radiant point' and velocity are known.

too quickly for exact reliance on any estimates of the velocity with which they traverse our atmosphere.[1]

Meteoric astronomy had reached this stage when Schiaparelli, who had already directed much attention to the subject, and had speculated somewhat boldly upon it, was led to compare the motion of the great comet of 1862 (No. III.) with that of the August meteors. Observing that this comet passed close to the earth's orbit on a course somewhat resembling that of the August meteors (on the assumption that they are moving with a velocity exceeding the earth's), he inquired whether the agreement would be found yet closer if the actual velocity of the comet where it passes close to the earth's orbit were assumed to be that with which the meteors are travelling when they enter the earth's atmosphere.

As this was the sole assumption made by Schiaparelli, it will be well to consider how far it affects the probability of inferences based on the coincidence actually recognised when this assumption has been made.

The real velocity of the August meteors cannot possibly exceed twenty-seven miles per second; since any greater, where they cross the earth's track, would give them a hyperbolic orbit, whereas it has been shown that they form a closed ring. Again, the real velocity cannot greatly fall short of the earth's, since otherwise the meteors of the system would appear to come from a point lying much nearer to that part of the heavens towards which the earth is travelling on August 11 than is actually the case.

Thus, let E E′ be a part of the earth's course on August 10, Σ' the point on the heavens towards which she is travelling at the moment. Let E *l* be a line directed towards the radiant point of the August meteors, which lies about 40° from Σ′. Then E′ *m* perpendicular to E *l* represents the least possible velocity for the meteors (where E E′ represents the earth's velocity). Coming from the direction of a point *s* in the heavens, with this velocity they would seem to radiate from Σ, E′ Σ being drawn parallel to E *l*, since a meteor which was at *m* when the earth was at E would meet the earth at E′. Again, if we take E *m*′ equal to the diameter of a square of which E E′ is a side (that is equal to E *k*), this line will represent the greatest velocity a meteor crossing the earth's orbit can possibly have if it is travelling in a closed orbit.[1] This gives the direction *s*′ *m*′ E′, along which an August meteor would actually travel, on the supposition that it has this maximum velocity. We may fairly assume

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[1] In any exact investigation of meteoric motions the influence of our earth's attraction, as well as the effects of the earth's rotation—the former producing a real, the latter an apparent, change in the paths of meteors—must be taken into account. These circumstances do not, however, affect the general reasoning given above, and their effects are unimportant compared with those due to the circumsolar motions of the meteors and of the earth.

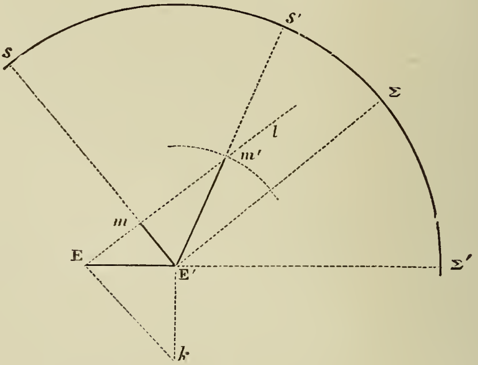

FIG. 3.—Showing how the arc on the heavens on which lies the true radiant of the August meteors can be determined by a simple construction.

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[1] Because if v be the velocity of a body moving in a circle at a distance d from the sun, it can be shown that the velocity in a parabolic orbit will be $\sqrt{2} \, . \, v$, at a distance d from the sun. The earth's orbit is near enough to a circle to render this relation applicable ; moreover, on August 10 the earth is nearly at her mean distance from the sun.

that the meteors are not arriving on a course intermediate to *m* E′ and E E′, because then their relative velocity would fall much too far below the velocities estimated by Secchi, Newton, Herschel, and others.

From some point on the arc *s s′* upon the celestial vault, therefore, the August meteors are certainly travelling; and with some velocity intermediate to the velocities represented by the lines *m′* E′ and *m* E′, where E E′ represents a velocity of 18½ miles per second. Moreover, the reader is not to suppose that the figure deals with hypothetical relations, and does not admit of being at once and definitely interpreted. Σ′ represents a real point on the heavens, a point on the ecliptic close by the star δ Arietis, towards which, as already mentioned, the earth is actually travelling on August 10. This point can at once be found on a celestial globe. Σ, again, is a known point, the radiant of the August meteors (already indicated), and about 39° from Σ′. It can, of course, be found on a globe, since its R.A. and declination have been given. Now let the end of a cord be held down on a globe at the point corresponding to Σ′, and let the string be stretched over the globe across the point corresponding to Σ, so as to pass beyond Σ. It will indicate the position of the arc *s′ s* on the celestial sphere. It will lie in a circular arc of which Σ Σ′ will represent 39°. And if we take points on it corresponding to *s* and *s′*; in other words, so that *s′* falls, as shown in Fig. 3, about 26° from Σ, and *s* 90° from Σ, we shall have on the celestial globe the arc *s s′*, along which the true radiant of the August meteors must lie. In other words, the August meteors are really travelling, when they cross the earth's orbit, as if from a point, this point lying somewhere on an arc about 64° long, and having the same place upon the star-sphere that the string arc corresponding to *s s′* has upon the celestial globe.

Thus far there has been no assumption whatever. Moreover, without proceeding a step further, we can indicate a surprising coincidence which Schiaparelli was the first to recognise. The great comet of 1862 did beyond all question cross the earth's orbit from the direction of this very arc *s s′*, that is, as if travelling from a point lying somewhere on this arc. The elements of the comet as determined by Oppolzer sufficed to demonstrate this. Thus, without any assumption whatever, it was proved that comet and meteors reached the part E′ of the earth's orbit along lines contained within one and the same sector of space, *s* E′ *s′*.

But Schiaparelli had already indicated reasons for believing that the velocity of the meteors was nearly equal to the maximum (indicated in Fig. 3 by the line *m′* E′). What he now did was to point out that, granted this assumption only, the point whence the meteors are directing their course as they cross the earth's orbit lies close to *s′* (on the side away from Σ, of course), and that the point whence the comet was directing its course when it crossed the earth's track had appreciably *the same position*,—the comet's velocity, on this assumption, being identical with the meteor's velocity.

So that to sum up—*Assuming nothing, it was demonstrated that a comet and a meteor system, crossing the earth's track at the same spot, have directions certainly so far agreeing as to lie in the same sector in space—that sector having an angle of only* 64°; and, *assuming for the meteors a velocity corresponding to what had already been assigned as the probable value of this element, the comet and meteors cross the earth's track on one and the same course, and with identical velocities*.

This in reality indicates the full extent of the coincidence recognised by Schiaparelli. We must not go further, as some have done, and say that the orbit ring of the meteors was further found to coincide perfectly with the orbit of the comets. Still less must we (*as an additional proof of coincidence*) set the elements of the orbits side by side, as though the agreement of the two in respect of eccentricity, inclination, longitude of node and of perihelion, and so on, afforded so many additional evidences of coincidence. The fact is that, setting aside Schiaparelli's assumption as to the meteoric velocities, there is no agreement as respects any of these elements except the longitude of the node. There is a further coincidence not directly expressible in the ordinary orbit elements, and this coincidence, as indicated above, is sufficiently striking. It justifies, in fact, the assumption of equal velocities. *Then*, this assumption being made, and leading to the coincidence of the actual direction of the two orbits where they cross the earth's, the exact coincidence of the two orbits, in all respects, follows inevitably,—since two bodies moving with equal velocities and in the same direction as they cross one and the same point in interplanetary space, must, by the law of gravity, follow identical orbits.

Or the matter may be otherwise expressed thus :—It follows from a discussion of the motions of the comet of 1862, that if the earth had been close by the place where her orbit is crossed by the comet when the comet actually traversed that place the comet's course would have seemed to be directed from the radiant of the August meteors. So much is *certain*. Schiaparelli inferred that the actual course of the comet as respects velocity and direction was identical with the course of the August meteors as they traverse the earth's orbit. We have already had occasion to consider the probability of such an inference, in speaking of the probable association between all the meteors which on any night appear to have the same radiant. The chances are great against the coincidence being accidental.

If I were here dealing with the history of meteoric astronomy, I should have to give a full account of the recognition of a corresponding resemblance between the motions of the November meteors and those of Tempel's comet (No. I., 1866). In particular, it would be desirable to discuss the share which Professor Adams took in the work. I notice with regret, by the way, that in Dr. Schellen's useful work on 'Spectrum Analysis,' the labours of Adams are left wholly unrecognised, while the comparatively less important researches of Schiaparelli, Oppolzer, Peters, and Leverrier are pointedly referred to. This is not the place to supply the omission; but I may remark that if we set aside the labours of Adams, the only circumstances in which the discussion of the November meteors differed from that of the August meteors were *these*: there was, first, a reason for assigning a particular period to the November meteors, in the circumstance that great displays of these shooting-stars occur at mean intervals of about 33¼ years, and assigning a period amounted to assigning the velocity with which the meteors cross the earth's track; and secondly, the comet which agrees in its motions with the November meteors was dealt with separately, and the agreement only recognised after both orbits had been independently worked out. But the choice of 33¼ years as the period of the November meteors was still an assumption; and there were many eminent astronomers who regarded 34-33rds, or 32-33rds of a year as the more probable period. In any case, assuming a period, the task of determining the orbit from the observed position of the radiant was mere mathematical child's-play. But Professor Adams achieved a much nobler work. For he proved from the observed displacement of the node of the November meteors—that is, of the point where the meteors cross the earth's track—that the period *must* be 33¼ years or thereabouts. This was a task which none but a mathematician of the highest order could possibly have accomplished; and even to such a mathematician the task was a most laborious one. I do not say that Schiaparelli's case was not made out without Adams's assistance. The chances against the observed agreement as a result of accident were as great in the case of the November meteors as in the case of the August meteors, and the chances against both coincidences being accidental were therefore overwhelming. But Adams undoubtedly removed whatever element of doubt still remained; and, furthermore, if Schiaparelli's theory had never been started, Adams's work would of itself have sufficed to establish the true figure of the orbit on which the November meteors travel. It has seemed fitting to say thus much in recognition of the remarkable labours of our great English mathematician; but it need hardly be said that the value of Schiaparelli's ingenious researches and theories is in no way affected by the matter here referred to. The great importance of Schiaparelli's work consists in the light which it throws on cosmical problems of extreme interest and difficulty.

The fact, then, is demonstrated that two of the meteor systems encountered by the earth are so far associated with two comets as to travel on the same orbits. We may not unsafely infer that all the meteor systems encountered by the earth are in like manner associated with other comets. Nor is it very rash to assume that all comets are in like manner associated with meteor systems. Two cases may seem insufficient as a basis for such wide generalisations as these; but it must be remembered that they are the only two cases we have yet been able to deal with satisfactorily. It should be remarked, however, that some other comets have passed close by the earth's orbit, and that in several cases meteoric radiant points have been recognised which correspond fairly with the assumption that those comets are followed by meteor trains. However, as nothing has yet been proved respecting the relative length of meteoric trains following after comets, we cannot expect that the track of every comet should to any great distance be thus followed; and where the distance is relatively small, no evidence of the kind just referred to would be obtainable even a year or two (perhaps) after the comet had crossed the earth's orbit.

And here a word or two may be permitted on the question of the condition of comets freshly arriving on the scene of the solar system. It is assumed sometimes that the train of meteors already exists when the comet first comes within the solar domain. But, however this may be in other cases, there can be no question that in the August and November meteors, the train—at least the train we are acquainted with—must have been formed after the comet had become a member of the solar family. This will be evident if we consider how this last-mentioned result can alone come about. Take the case of the November meteors, the figure of whose orbit shows that the parent comet was forced to become a regular attendant of the sun by the disturbing influence of Uranus. Now, the circumstances under which a comet approaching the sun on a parabolic or hyperbolic orbit can be thus affected must be regarded as exceptional. The planet's influence must in the first place be very energetically exercised ; in other words, the arriving comet must pass very close to the planet ; for under any other circumstances the sun's influence so enormously outvies the planet's that the figure of the cometic orbit would be very little affected. Moreover, the planet's attraction must produce an important balance of retardation. The planet will inevitably accelerate the comet up to a certain point, and afterwards will retard it : the latter influence must greatly exceed the former. To show how greatly the comet must be retarded, it is only necessary to mention that the actual velocity of the November meteors, when they cross the orbit of Uranus, is less than one-third of the velocity with which Uranus himself travels ; but their velocity at the same distance from the sun, when they were approaching him from some distant stellar domain, exceeded the velocity of Uranus in his orbit in the proportion of about 7 to 5. So that, roughly, their velocity at that distance has been reduced in the proportion of more than 21 to 5, or by $\frac{7}{9}$ of its original value. This is a reduction of about $4\frac{1}{2}$ miles per second. Now Uranus could barely impart this velocity to a body approaching him from an infinite distance under his sole influence, and coming as near to him as the nearest of his satellites ; but he could not impart so much additional velocity to a body approaching him (more rapidly of course) under solar influence. Now, the velocity a body can impart it can also take away. Hence the retarding action of Uranus could under no circumstances account for the velocity of about 4½ miles per second, by which the motion of Tempel's comet must have been reduced, unless that comet passed closer to Uranus than his nearest satellite. Indeed, the maximum velocity which Uranus could impart to a body actually reaching his surface (and exposed to his sole influence in approaching from infinity) amounts only to 13·7 miles per second. Now the distance of Uranus's nearest satellite from the planet's surface is but about 84,000 miles, and Uranus traverses such a distance in less than five hours. So that the first and last meteors so influenced by Uranus as to be forced to part with a velocity of 4½ miles per second out of their actual velocity of about 6 miles per second, were assuredly not separated by a distance equal to a five hours' voyage, or in miles (at 6 miles per second) by so much as 108,000 miles. We may, indeed, safely infer that the actual distance was much less than this. For though all the meteors along a distance of 108,000 miles might have their velocity sufficiently reduced, yet in this case some of the meteors would have their velocity too much reduced, and would thenceforth pursue orbits differing very markedly from the orbits traversed by the remaining members of the system.

It follows, not merely as a probable inference, but I think as a demonstrated conclusion, that if the November meteors came originally into our system as a comet travelling sunward from infinity, then either that comet was very compact, or else Uranus captured only a small portion of the comet, the remaining portions moving thenceforth on orbits wholly different from the path of the November meteors.

There is no escape from this conclusion ; for no other planet than Uranus can have brought about the subjection of this comet to solar rule.

One is almost led to doubt the extra-planetary origin of the November meteor system altogether, and to entertain the theory that the comet which produced it was in the first instance expelled from Uranus. We need not conclude, if we accept such a view, that the period named by Leverrier for the introduction of the comet into the solar system (A.D. 126) was necessarily the epoch of the expulsion of the comet by Uranus. The true epoch of this event may have been far more remote.[1]

This, at any rate, is certain, that if the comets belonging to our system have been introduced, as is commonly supposed, by planetary perturbing influences, then the actual number of arriving comets must have indefinitely exceeded the number captured in this way. For an arriving planet has a million chances of escaping for one of being captured—so closely must it approach to one of the major planets if its motion is to be sufficiently controlled to cause it to become a permanent member of the solar system, with a perihelion near enough for the inhabitants of earth to recognise the comet, and with an aphelion not greatly beyond the orbit of the disturbing planet.

But be this as it may, the connection between meteors and comets remains an established fact, the existence of many comets in the solar system is a reality, and the fact that the earth encounters more than a hundred meteor systems cannot be disputed.

Now, if we consider what proportion of interplanetary space the earth really traverses, we shall begin to recognise one of the most surprising of the conclusions which are deducible from these demonstrated facts. If the earth were really, as she is sometimes pictured, a globe so large that in an ordinary picture of her orbit around the sun she would be presented, to scale, as a considerable sphere, there would be nothing very remarkable in the circumstance that on her course round the sun she should come into contact with many meteor systems. But when we are reminded that on so large a scale that the earth's orbit would be represented by a circle *ten feet* in diameter, the earth herself would be but about the 190th of an inch in diameter, so that the path her globe actually traverses would be represented by a circle 10 feet in diameter, and marked in with about as stout a stroke as the down strokes of the letters in this page, we see at once how minute a portion of sun-surrounding space this ring-orbit really occupies. Remembering that nearly all the meteor systems encountered by the earth travel on orbits largely inclined to the plane of the ecliptic, and, therefore, cross that plane in two spots (unequally distant in nearly every case from the sun), it will be seen how wonderful the circumstance is, that some 200 such spots should fall on the fine circle which forms the earth's true track in the ecliptic.

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[1] It is also possible to account for the present position of the November meteor system by supposing that the encounter took place during that remote period when Uranus (according to the nebular theory) occupied a much larger region of space than at present.

Fig. 4 is intended to illustrate the remarkable nature of this circumstance. Here the circle E_1 E_2 E_3 E_4 represents the earth's path around the sun, E being the place of the earth at the time of the winter solstice. If we suppose the width of the circular line E_1 E_2 E_3 E_4 reduced to about a thirtieth part of its present value, we may regard this circle as picturing the actual shape of the region traversed by the earth. Now we know very little as to the real extent of the hundreds of meteoric rings traversed by the earth ; but the cross section of each (regarded as cylindrical where crossing the ecliptic) may average perhaps a million or a million and a half of miles. According to the inclination of each and the actual direction of motion, the cross section with the ecliptic plane will be an ellipse of greater or less eccentricity, and having its longer axis in such and such a direction (which may be any whatever) with respect to the direction of the earth's orbit at the place of transit. These elliptical sections must, therefore, have some such arrangement as is depicted in Fig. 4. And it may be noted that, for my present purpose, it is a matter of no importance whether the cross sections of the meteor rings be exaggerated or the reverse : since, on the one hand, if the cross sections are really larger, the importance of the several meteor zones is enhanced, but the circumstance that any given meteor zone is encountered by the earth is rendered *pro tanto* less surprising; while, on the other hand, if the cross sections are really smaller, we must infer that the number of meteoric zones is proportionately greater, to give the earth a reasonable chance of encountering so many systems. As to the shape of the cross section, it matters little what opinion we form; only if the real cross section is circular, the cross section on the ecliptic must have different oval shapes as shown in fig. 4.

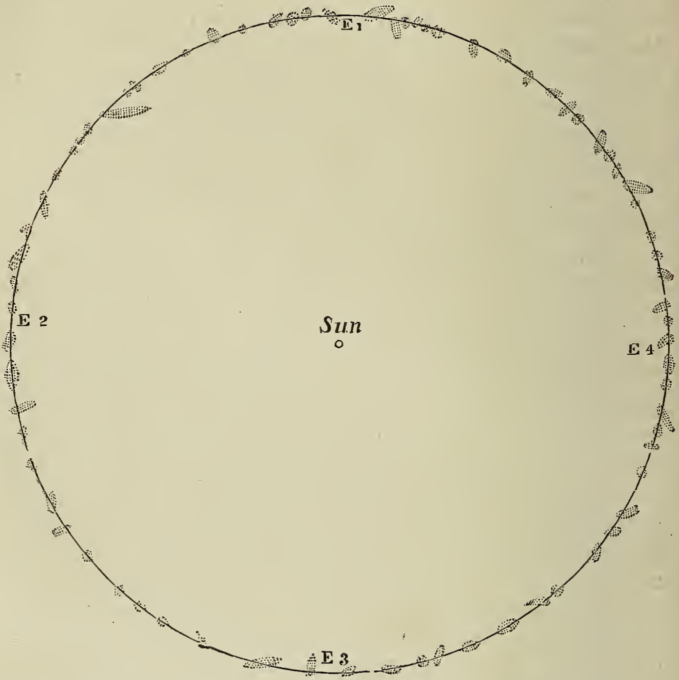

FIG. 4.—Ideal view of the ecliptical cross sections ot about a fourth of the known meteor systems.

So far, then, as the meteoric systems which cross the ecliptic *descendingly* are concerned, we may fairly assume that their cross sections on the ecliptic form a scheme somewhat resembling what is pictured in the figure. It is known that southern skies are as plentiful in meteors as northern skies, if not more so ; hence we must infer that as many cross sections as are depicted in fig. 4 should be added (all overlapping the circle $E_1 E_2 E_3 E_4$) to represent the meteor systems which cross the ecliptic *ascendingly*, or from south to north. Moreover, an addition should be made for meteor systems which have hitherto escaped notice, as well as for a considerable number (perhaps nearly as many again as all yet mentioned) which, because they strike the earth's course on its inner or sun-illumined side, fall on the earth where day is in progress, and so escape recognition altogether. Add also the more sparse systems of 'heavier metal' which produce fire-balls, and those others which have supplied the countless myriads of aërolites which are known to have fallen on the earth. I think it will be granted that, if all these circumstances were taken into account in Fig. 4, the earth's orbit, as there pictured, would be absolutely encumbered with the oval spots representing the cross sections of meteoric systems.

Now let it be remembered that each of the cross sections corresponds to a long stream of meteors, if not to a complete zone, the meteors travelling around orbits compared with which the orbit $E_1 E_2 E_3 E_4$ has utterly insignificant proportions. Imagine the numerous cross-sections of fig. 4 replaced by as many curves carried on various arcs, even only across the circular space $E_1 E_2 E_3 E_4$ (that is, leaving out of account altogether those portions of the systems which lie farther away from the sun). Conceive the like done for about twice as many more systems corresponding to the orders just mentioned. This considered, it will surely begin to appear that the *sphere* of space around the sun, which $E_1 E_2 E_3 E_4$ will represent, is occupied in a remarkable manner by interlacing meteor-comet systems.

But it is certain that the earth's orbit is not clustered round with meteoric cross sections in the peculiar manner depicted in fig. 4. That all those cross sections are there cannot be questioned, since so many northern meteor systems have been recognised. But outside and inside $E_1 E_2 E_3 E_4$ there must be meteor cross sections which the earth does not traverse. To suppose otherwise is as though a person who had traversed a certain route in a rainstorm should suppose that no rain had fallen to right or to left of his track. There is nothing in the earth's orbit to attract meteors. She herself has not the attractive energy necessary either to compel meteors to approach her track or to retain them against other attractive influences. It is only by chance, as it were, that her track lies thus through those special hundreds of meteor systems,—precisely as it is only by chance that such and such raindrops fall upon our illustrative traveller.

Hence, we have no choice but to suppose that the whole plane space within the circle $E_1 E_2 E_3 E_4$, as well as an immense plane extent of space outside that circle, is as richly bespread by meteor systems as we have seen to be the case with the circular track $E_1 E_2 E_3 E_4$—that is, much more richly than as pictured in fig. 4. And here, too, we recognise the small importance of the extent we have given to the meteor cross sections in fig. 4; since, if the extent of each were smaller, we should have to cover the space within and without with greater numbers of these smaller cross sections, in order to account fairly for the fact that the earth's track lies across so many of them.

But when we remember, further, the connection which Schiaparelli has shown to exist between meteor systems and comets, we find a reason for believing that not merely a uniform degree of meteoric richness continues inwards from the earth's orbit up to or near the very place of the sun, but that a great increase of richness takes place as the sun's place is approached. Mr. Dunkin, in his valuable Appendix to Lardner's 'Astronomy,' presents the actual densities with which the perihelion points of observed comets are distributed throughout sun-surrounding space. He gives the following table :—

Distance from Sun in Millions of Miles	Number of Perihelia	Relative Cubical Space	Density of Perihelia
0 to 20	8·65	1	8·65
20 to 40	11·70	7	1·67
40 to 60	20·50	19	1·06
60 to 80	17·20	37	0·47
80 to 100	20·80	61	0·34
100 to 120	8·65	91	0·10

Here it will be observed that the increase of density with approach towards the sun is very rapid indeed in the sun's immediate neighbourhood. It is represented by the ordinates of the curve c c′ in fig. 5, the dotted part of the curve being that inferred from the shape of the part which has been determined by observation. Moreover, it should be noticed that comets having their perihelia between 40 and 100 millions of miles from the sun are, on the whole, more likely to be recognised than those whose perihelia lie nearer to the sun.

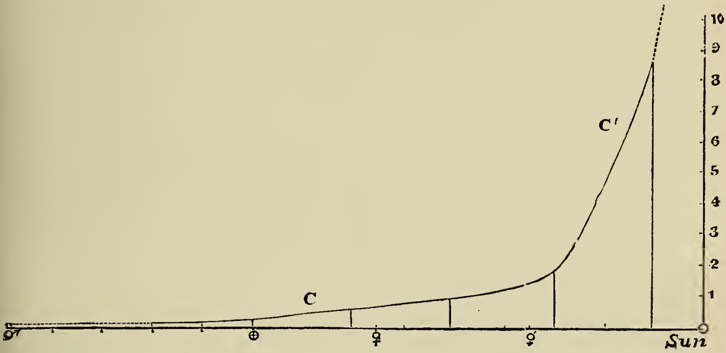

FIG. 5.—Illustrating the increase in the richness of cometic distribution with approach towards the sun.

We may fairly infer that the law indicated here for comets is that which holds also for meteor systems. Now let the following reasoning be carefully noted :—The members of a meteor system in travelling towards their perihelion open out their ranks, as it were, because travelling there with continually increasing speed, so that a given time difference between the place of two meteors corresponds to a continually increasing distance. We overrate this opening out in taking it as proportional to the square of the distance from the sun. Now the volumes of spherical spaces around the sun vary as the cube of the distance within which they are enclosed. Hence, *cæteris paribus*, the richness of meteoric distribution around the sun, being proportional to $\frac{\text{number}}{\text{volume}}$, must vary inversely as the distance. Thus, a set or group of meteors, which at a distance of 90 millions of miles (about equal to the earth's) would spread with a certain degree of richness, would at a distance of 10 millions of miles be spread nine times more richly. Now, the above table shows that at a mean distance of 10 millions of miles (taking this as corresponding to the limits 0 and 20 millions—an assumption very unfavourable to the argument) we have a density of perihelion distribution represented by 8·65 as against a density of only 0·34 at a distance of 90 millions of miles. Thus the density of perihelion distribution is about 25½ times greater at the former distance; and the actual mean meteoric density is about (9 × 25½, or) 230 times greater. Further, the illumination of these meteoric bodies at the lesser distance is 81 times greater, since this illumination varies as the square of the distance. Hence, under equally favourable conditions, the total quantity of light reflected from meteors within a given considerable space, at a distance of 10 millions of miles, exceeds that reflected from a set within an equal space at the earth's distance, in the proportion of 230 × 81 to 1, or upwards of 2,000 times. Nearer to the sun than this still enormous distance the quantity of reflected light must be vastly greater; and if any meteors become incandescent owing to the great heat to which they are exposed, the total amount of light from these sun-surrounding regions must be yet further increased.

It should be noticed that the only assumption which has been made in the above reasoning is so far in accordance with the evidence actually obtained that any other assumption would have a considerable weight of probability against it. For if Schiaparelli's discovery has any cosmical importance at all—and everyone admits that it *has*,—it implies that all comets are followed by meteoric trains.

The reader has only to turn to fig. 4, and to conceive the meteoric cross sections marked in all over the circular space E_1 E_2 E_3 E_4 with that degree of increasing richness *sunwards* which has been indicated above (remembering also that, as at present drawn, the figure shows less than a fourth the real number of cross sections overlapping the earth's orbit), to perceive how richly sun-surrounding space must be crowded with meteoric systems. For not the ecliptic plane alone, but every plane through the sun, must be similarly intersected.

Albeit we must not forget that the meteor systems severally are almost infinitely tenuous. It has been calculated by Professor Newton, of America, that even on the occasion of the great display of November meteors in America in 1867, the portion of the system actually traversed by the earth contained only one meteor in 900,000 cubic miles of space—that is, in a cubic space nearly 100 miles long, broad, and deep; so that, even taking into account the greatly increased richness close by the sun, we have not to deal with a real crowding of cosmical material, but, on the contrary, with an excessive tenuity, using this word to indicate the relation between the quantity of matter (however distributed) and the volume of the space within which it exists.

The reader will doubtless have surmised already the special purpose which I have had in view in the preceding inquiry. It seems to me that we have in meteoric phenomena, as well as in the associated phenomena of comets, the explanation of some at least of the features presented by the solar corona. I cannot see how, on the one hand, the irregularities of structure which the corona presents at great apparent distances (up to two or three solar diameters, for instance) can be accounted for except by the theory that during eclipse the complicated network of meteor systems becomes discernible; nor, on the other, how the meteor systems can by any possibility escape recognition when a total solar eclipse is seen under favourable atmospheric conditions.

It has been supposed that, because I have advocated another theory in explanation of other features of the corona, I have abandoned the meteoric theory which I had formerly advocated. It is true that, in general, to advocate a new theory implies that a theory formerly held has been abandoned. But, in the present instance, this is not the case. The solar corona is a complicated phenomenon, and presents features which are severally due to different causes. Its irregularities may reasonably be attributed to one cause, while such features as the straight radial extensions, rifts, and so on, may be (or rather must be) ascribed to another. It may be compared, in this sense, with the aurora. In explaining the general nature of this phenomenon, we may call into our aid the meteoric particles continually descending, in an irregular manner, through the upper regions of air; but in accounting for the auroral streamers we have to consider processes taking place along straight (possibly along radial) lines.

It has been objected to the meteoric explanation, that the parts of the corona near the sun do not present the appearance which we should expect to recognise in meteors close by the sun, and are furthermore much brighter than we could expect even the innermost parts of the meteor region to appear. In this reasoning the circumstance seems to be overlooked that the meteor light which seems to come from regions close by the sun (assuming the meteoric theory to be true) does not come wholly, nor even chiefly, from meteors really so close to the solar orb. We look through a range of meteors two or three hundred millions of miles deep (taking into account space beyond the earth's orbit), and it is the combined effect of the light coming from the whole of this enormous range that we really recognise,—*not* in the corona, but in that proportion of the coronal light which is due to sun-illuminated meteors. The part of the range nearest to the sun may be the part most densely crowded and most brilliantly illuminated; but its extent is limited compared with that of the whole range; moreover, the meteors *there* situated turn but half-discs (of reflected light) towards the earth, those beyond showing a much larger proportion of their illuminated halves. It is worthy of notice, indeed, that the farther half of the range supplies much the larger proportion of the light, on account of the greater fulness of illumination,—for, in such a case as this, distance *per se* is an element which may be absolutely neglected.[1]

It need scarcely be pointed out that the spectroscope affords the best means for testing this question. If any considerable proportion of the corona's light is reflected from meteors, this portion of the light should exhibit a solar spectrum, though of great faintness; or, unless great light-gathering power were employed, a faint continuous spectrum would be seen. The zodiacal light, also, should exhibit a continuous spectrum if it represents the outer portion of the sun-surrounding meteor families. Until within the last few months the coronal light had been known to give a continuous spectrum as well as certain bright lines (or one bright line); and it had been stated that the zodiacal light gives a bright line spectrum. The first evidence was questionable; the second seemed opposed to the meteoric theory. But Janssen has examined the coronal light with the most powerful light-gathering means yet employed, and he recognises the solar dark lines in its spectrum. This evidence is unquestionable. And Liais, in the clear skies of tropical South America, has examined the zodiacal light, and gets an infinitely faint continuous spectrum, so that what seemed a strong objection to the meteoric theory is removed. Let us pause, however. Liais has been charged with drawing an ideal picture of the corona during total eclipse (his drawing, by the way, singularly countenancing the meteoric theory). But it was ideal : how, then, shall Liais's evidence be trusted on any other subject? What, however, if it was not ideal at all, but simply characteristic, because Liais observed the eclipsed sun under singularly favourable conditions at his southern stations in 1858? This is precisely the inference fairly deducible from (or rather the conclusion forced upon us by) the evidence of the observers of the recent eclipse. Janssen speaks of special forms resembling those seen by Liais ; observer after observer speaks of complicated structures within the corona ; the photographs tell the same tale.

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[1] This at first sight may sound paradoxical; but it is strictly true nevertheless. The question is one of the apparent brightness of certain regions of the heavens, not of the total quantity of light received from given groups of meteors. A group of bright objects so far off as to appear like a cloud would preserve its *brightness* absolutely unchanged, however far off the observer might remove. Its extent alone would diminish.

In conclusion, I believe little question can exist that a large proportion of these phenomena which have seemed most perplexing, as well in the solar corona as in the zodiacal light, admit of being very readily explained when studied in the light of the now accepted theories of meteoric astronomy.

COMETS.

THE year (1881) which was to have seen the end of the world, because of planetary conjunctions and perihelion passages, because Mother Shipton had said so (or was said to have said so), and because the ascending gallery in the Great Pyramid is 1,882 inches long (so that the year 1882 is to introduce a new era), has been remarkable in astronomical annals for the number of comets which have been seen. No less than six were numbered, two of which were of great brightness. Something still remaining—more, indeed, than we are always ready to admit—of old superstitions respecting comets has led many to regard the coincidence as full of meaning. Others, not quite so credulous, have supposed that though comets may not come in flights of half a dozen together to portend the end of the world, they may yet affect our weather in some way; perhaps directly, as the moon is supposed to do (with very little reason); perhaps indirectly, by acting on the sun. To the astronomer the appearance of so many comets—some of them large ones—has been full of interest, because he hopes by the application of the new methods of research discovered within the last quarter of a century to solve some of the mysteries with which the whole subject is still fraught, despite a number of interesting discoveries which have recently been made.

A brief inquiry into some of the facts which have been discovered respecting comets, and a discussion of some of those peculiarities which still remain among the greatest mysteries of science, will probably prove acceptable at the present time, when comets attract so much interest and attention.

Elsewhere in the solar system we meet with relations not differing greatly in kind from those presented by our own earth. We see a set of globular bodies revolving around the sun in nearly circular orbits, nearly in one plane, and all in the same direction; we find that these globes rotate upon their axis—still in the same direction; they have, apparently, atmospheres proportioned to their dimensions; and many of them are attended upon by bodies resembling our own moon. And therefore, without entering upon the vexed question of the plurality of worlds, we are able to pronounce that, *if* these globes are inhabited, dwellers upon them have, like us, their years, their days, their seasons; a sun—rising in the east and setting in the west; twilight and moonlight; air and vapour; winds and rain; all things, in fact, as it would seem, necessary to their comfort and convenience. Here and there—as in the zone of asteroids and the rings of Saturn—we meet with novelty of arrangement or configuration; but even then we find a stability, either of figure or motion, which renders such objects comparable, so to speak, with those we are accustomed to.

But with comets the case is wholly different. When we have said that these objects obey the law of gravity, we have mentioned the only circumstance—as it would appear—in which they conform to the relations observed in terrestrial and planetary arrangements. And even this law—the widest yet revealed to man—they seem to obey half-unwillingly. We see the head of a comet tracing out systematically enough its proper orbit, while the comet's tail is all unruly and disobedient.

The paths followed by comets show no resemblance either to the planetary orbits or to each other. Here we see a comet travelling in a path of moderate extent and not very eccentric; there another which rushes from a distance of two or three thousand millions of miles, approaches the sun with ever-increasing velocity until nearer to him than parts of his own corona (as seen in eclipses), sweeps around him with inconceivable rapidity, and makes off again to where the aphelion of its orbit lies far out in space beyond the most distant known planet, Neptune. Some comets travel in a direct, others in a retrograde, path; a few near the plane of the earth's orbit, many in planes showing every variety of inclination. Some comets regularly return after intervals of a few years; some after hundreds of years; others are only seen once or twice, and then unaccountably vanish; and not a few show by the paths they follow that they have come from interstellar space to pay our system but a single visit, passing out again to traverse we know not what other systems or regions.

The ancients believed comets to be of the same nature as meteors, or shooting stars—either in the earth's atmosphere not far above the clouds, or, at all events, much lower than the moon. These views are, however, much less ancient than the more correct views maintained by the Pythagoreans. Their doctrine was that comets are planetary objects, having long periods of revolution. From whom this opinion was derived is uncertain. Like other opinions attributed to Pythagoras, it was doubtless obtained from Eastern philosophers; but of what country—whether Egyptian, Persian, Indian, or Chaldæan—we have no means of learning. Apollonius, the Myndian, ascribes the opinion to the Chaldæans. He says they spoke of comets as of travellers penetrating far into the upper (or most distant) celestial spaces. Seneca and Pliny held similar views, exhibiting in this respect, says Humboldt, the imitative faculty of the Romans. But the Greek philosopher preferred to look for a theory of the universe in the conceptions of his own brilliant and imaginative mind. As if to show future ages how little was likely to be achieved by the highest mental powers without the habit of patient observation, he endeavoured to educe a system of philosophy from fancies, and to found it upon syllogisms. Aristotle, who may be considered the typical philosopher of the Greek school, included comets in the wide range of phenomena which he claimed the privilege of explaining. To him was due the opinion mentioned above—an opinion confidently maintained during the many centuries in which the philosophy of Aristotle held sway over men's minds. To him, also, was due a yet more remarkable opinion, the view, namely, that the Milky Way is a vast comet which continually reproduces itself! Xenophanes and Theon, in the fifth century, adopted a rather singular view of the Aristotelian theory of comets, when they spoke ot these objects as 'travelling light-clouds.'

To these fancies the ancients added the idea that the shapes of comets indicated their character as portents. Thus in fig. 1 five views of comets are shown, as an arrowhead, a sea monster, a sword, a lance, and in flames.

Tycho Brahe was the first to express doubts respecting the views of Aristotle. From a careful series of observations, he demonstrated that the orbits of comets are certainly situated beyond the moon's orbit. He thought the orbits must be circular, for he lived at a time when none but circular orbits were conceded to the celestial bodies. Dörfel, a native of Upper Saxony, proved that the orbits of comets are either very elongated ovals or parabolas, and that the sun occupies a focus of the curve. It happens, singularly enough, that this discovery was effected but a year or two before Newton propounded the theory of gravitation. Newton himself examined the orbit of the great comet of 1680 (known as 'Newton's comet') and others; and he found that they all accord with the law of gravity.

But before long, Newton's friend and pupil, Halley, effected a yet more remarkable discovery. In hopes of confirming Newton's views by results founded on actual observation, he collected all the records of comets which seemed entitled to confidence, and attempted—as well as his meagre materials would allow him—to calculate the elements of their orbits. In this way he computed the paths of no less than

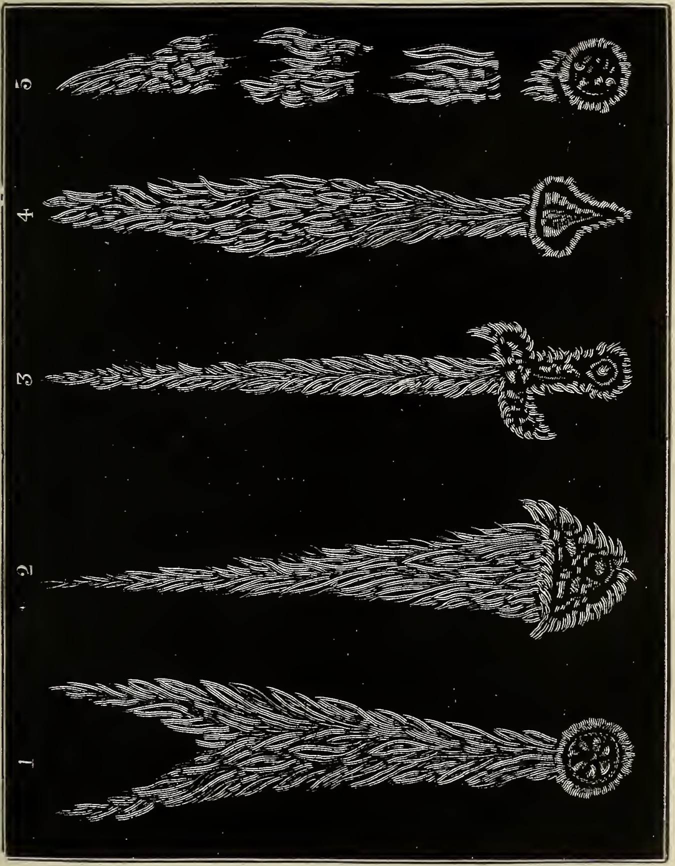

Fig. 1.—Various fanciful views of comets, according to Pliny. From the 'Cometographia' of Hevelius.

twenty-four. Among these, three presented a remarkable similarity. One appeared in 1531, and was described by Appian; another appeared in 1607, and was observed by

Kepler; the third was traced by Halley himself in 1682. The equality of the intervals between these epochs led to the suspicion that the same comet had appeared three times And Halley found, on searching historical records, that a comet appeared in 1305, another in 1380, and a third in 1456. Combining these appearances with those mentioned before, he thought he had satisfactory evidence of identity. For he was sufficiently familiar with the results which might be expected to flow from the law of gravity, to be aware that *absolute* regularity of motion was not to be expected in a body traversing the solar system in an eccentric orbit, and swayed from its proper path by the attraction of such giant planets as Jupiter and Saturn. Indeed, it happens, singularly enough—one out of many remarkable coincidences in the history of comets—that the comet of 1380 was *not* Halley's comet, which really appeared in 1378, a date bringing in a yet greater discordance in the intervals than Halley had suspected and accounted for. With remarkable acumen—since no means existed in his day for anything like accurate computation—he not only pointed out the possible influence of the great planets in disturbing the comet in past revolutions, but he made a rough approach to an estimate of the effect that they would have on the period of its next visit. 'Instead of appearing in August 1757, as it would if its period remained unaltered, it will not appear,' he said, 'until the end of 1758 or the beginning of 1759, for it will be retarded by the action of Jupiter. Wherefore,' he adds, with a pardonable anxiety to secure the credit of his ingenious investigations, 'if it should return, according to our prediction, impartial posterity will not refuse to acknowledge that this was discovered by an Englishman.'

As the time for the fulfilment of the prediction approached, an intense interest was excited in the minds of astronomers. In 1757, Clairut, Lalande, and Madame Lepaute undertook the computation of the epoch at which the comet might be expected to return. They applied methods of investigation invented by Clairut himself. It resulted from their laborious computations that April 13, 1759, was fixed on for the epoch at which the comet should attain its closest approach to the sun, or, as it is technically expressed, should pass its perihelion. But Clairut was careful to allow a month either way, on account of unavoidable omissions in the calculation, and for the effects of unknown forces, 'such as the action of some planet too far off to be seen' (a happy anticipation of modern discoveries).

And now the heavens were swept diligently by all the telescopists of Europe, each eager to be the first to announce the discovery of an object whose appearance or non-appearance was to confirm or to disprove the Newtonian theory. It was actually discovered, however, without telescopic aid, by a Saxon farmer, George Palitsch, on Christmas Day, 1758. It reached its perihelion on March 13, 1759, confirming at once the accuracy of Clairut's computations, and the justice of his caution in assigning rather wide limits of error.

It was now evident that comets travel, like the planets, in determined paths; and also that the investigation of their motions is a subject worthy the study of the ablest mathematicians, and sufficient to tax their highest powers. An account of their labours would be out of place in such a treatise as the present; but we recommend the subject to the notice of the astronomical student, as one of the most interesting chapters in the history of modern science.

One comet, however, discovered not long after astronomy had achieved this triumph, seemed at first to teach a different lesson. In 1770 a comet appeared whose path turned out to be, not a long oval or parabola, as had been the case with all the orbits yet examined, but an ellipse of moderate extent, and not very eccentric. The orbit lay also much closer than usual to that thin slice of space (so to speak) within which the planets are observed to move. Lexell, who computed the path, found that the period of the comet was about five and a half years. Its return was carefully watched for, *but no one has ever seen the comet since.* The cause of its disappearance, and also of its sudden appearance —for this was equally remarkable, when we remember that so conspicuous a comet could not have been circulating long in its small orbit without discovery—was carefully inquired into. The result was singular. On tracing back the path of the comet it was found that it must have passed very near to the great planet Jupiter. 'It had intruded,' says Herschel, 'an uninvited guest into his family circle—actually nearer to him than his fourth satellite.' Accordingly, the comet's path, originally a long oval, had been bent into a curve of less extent. Having once entered on this new path, the comet was free to follow it—always returning, be it noticed, to the point at which it had started on it—so long as Jupiter was not interfered with. But it happened, unfortunately for the stability of the comet's motions, that, after going twice round the new path, it again presented itself near Jupiter's track, when the planet (which had meanwhile gone once round his orbit) was not very far from the scene of his former encounter. He accordingly again exerted his influence upon the unfortunate comet, and this time dismissed it on a path which will not admit of its approaching the earth near enough to be seen.[1]

Let us return, however, to Halley's comet.

It so chances that the comet which was the first to show full obedience to the law of gravitation was one which exhibited in a very remarkable and significant manner the characteristics which distinguish comets from other heavenly bodies, and make them so mysterious to the student of science. At the return of Halley's comet, in 1836, all that had signalised the return in 1759 was repeated, but the mathematical triumph was far greater. Damoiseau, Rosenberger, and Pontecoulant calculated the comet's return to perihelion within two or three days, instead of a month, and the time when it passed this point of its orbit corresponded, within a few hours, to the mean of their several estimates. On the northern heavens, where it was first seen, the comet presented a remarkable appearance, with a long and brilliant tail stretching over an arc of many degrees upon the sky. When it had passed from our northern skies, it was carried (after a short interval, during which it was lost to view in the sun's rays) to the southern heavens. Sir John Herschel, and Maclean (Astronomer Royal at the Cape), were prepared to receive it; but when first observed by them it showed none of the features which made it so remarkable in our skies. It had no tail and scarcely any head. In fact, Sir John Herschel, in one account, says, that as first seen it could only be distinguished from a fixed star by its motion. The study of its gradual change of aspect from that time threw so much light on the nature of comets' tails and other appendages (or at any rate of that particular comet's tail) that Sir John Herschel, not accustomed to be over-confident, said there could be no doubt as to the true interpretation of the observed phenomena.

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[1] It must be noticed, however, that Leverrier, who very carefully re-examined the question, was led to deny the accuracy of the results recorded above. Admitting that Jupiter has twice disturbed the comet, he thinks there is no certainty (for want of sufficiently accurate observations) respecting either the original path of the comet, or that in which it is at present circulating unobserved—if, indeed, it has not been *absorbed* by Jupiter.

Most persons know that the name 'comet' is derived from the word *coma*, or *hair*, and is applied to celestial objects which appear to have a hairy appendage. Modern astronomers do not, indeed, use the word *coma* in this sense, but draw a distinction between the *coma* and the tail. There can be no doubt, however, that the part now called the comet's tail was that from which these objects derived their name. The word *cometa* or *cometes* is not a lately-formed one; but was used by Cicero, Tibullus, and other ancient writers, and it is worthy of notice that all the names applied to comets by the Romans had a reference to *hairiness*—*stellæ comantes*, *crinitæ*, *concinnatæ*, they are called by Ovid, Pliny, and Cicero. The last term—signifying stars which show a *curled* or *crisped* hairiness—would not be very applicable, by the way, to any comets that have appeared in modern times. The Chinese applied to comets the name *sui*, or 'broom.'

It might be supposed that the hairy, broom-like, or tail-like appendage, so commonly seen in comets, is really a distinctive feature of these comets. This, however, is far from being the case. A very large number of comets have no visible tails. We refer, of course, principally to telescopic comets; for very few comets which have been conspicuous to the naked eye have wanted this appendage.

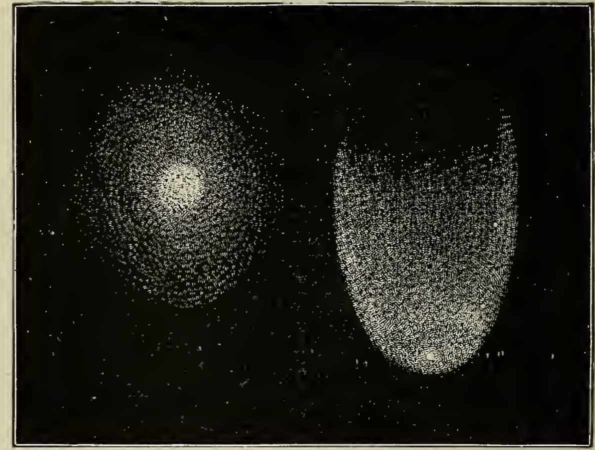

Fig. 2.— Changes of a Comet when first seen.

The *coma*—in the modern astronomical sense—is never wanting. This term is applied to a misty, hazy light, surrounding on every side a small bright spot, which is termed the *nucleus* of the comet.

When first seen in the telescope, a comet usually presents a small round disc of hazy light, somewhat brighter near the centre. As the comet approaches the sun, the disc lengthens, and, if the comet is to be a tailed one, traces begin to be seen of a streakiness in the comet's light. Gradually a tail is formed, which is turned always *from* the sun (fig. 2). The tail grows brighter and longer, and the head becomes developed into a coma surrounding a distinctly-marked nucleus. Presently the comet is lost to view through its near approach to the sun. But after a while it is again seen, sometimes wonderfully changed in aspect through the effects of solar heat. Some comets are brighter and more striking after passing their point of nearest approach to the sun (or *perihelion*) than before; others are quite shorn of their splendour when they reappear. The latter was the case with the comet of 1835–36, as we have already seen. On the other hand, the comet of 1861 burst upon us in its full splendour *after perihelion*-passage.

Some comets have more than one tail. One appeared in 1744 which had five or six double tails, symmetrically disposed (if one can trust the pictures handed down to us) in the figure of a half-opened fan (fig. 3). Others have presented a yet more peculiar appearance, having, besides a tail in the usual position, a second 'unconformable' tail, at right angles to the first, or inclined to it at some incongruous, out-of-the-way angle—for instance, in one case, one hundred and sixty degrees. Sometimes the peculiarity is presented of a perfectly dark gap separating the tail from the head. More commonly a dark space is seen behind the head, but on each side of this space the light from the head is continued so as to form a bright border on each side of the tail.

As a comet approaches the sun we have seen that a change takes place in the appearance of the coma and nucleus, and that in some instances a tail is generated. The process actually observed is generally this: in the forward part of the nucleus a turbulent action is seen to be in progress, leading to the propulsion towards the sun of jets or streams of misty-looking matter. Sometimes a regular cap or envelope is seen to be projected in this manner towards the sun, or even a set of envelopes one within the other. The matter thus thrown off is not suffered to pass very far from the nucleus towards the sun, but is swept away, as fast as formed, in the contrary direction. If the funnel of a

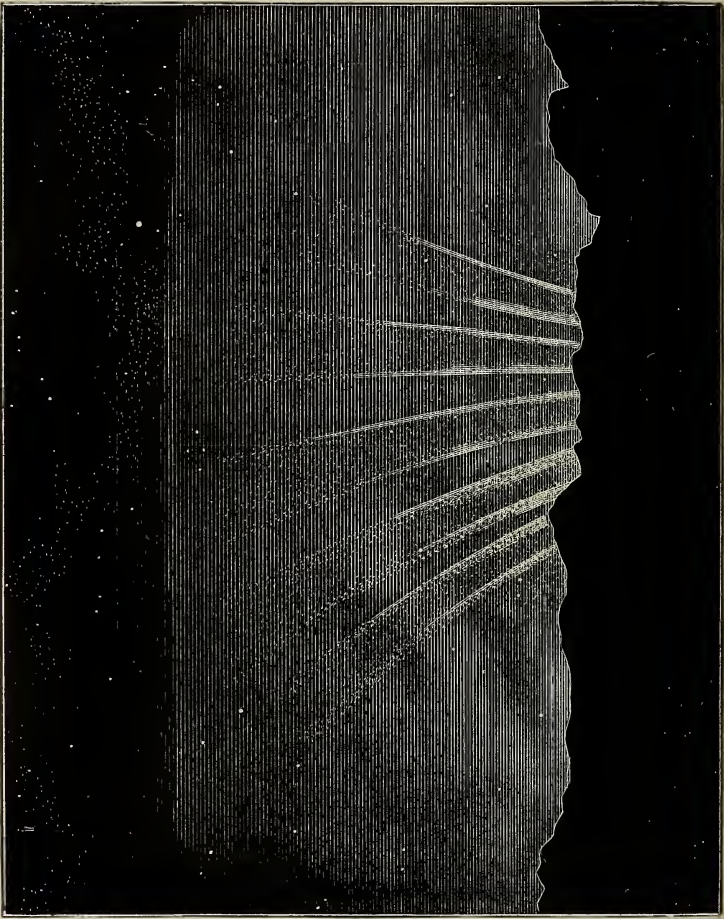

FIG. [illegible].—The Comet of 1744 (Cheseaux).

steam-engine were directed forwards, instead of upwards, then the appearance presented by the emitted steam, as the engine rushed on (against a hurricane, suppose, to make the illustration more perfect) would exemplify the process which seems to be taking place around the front of the nucleus, and far behind it, as the matter formed is continually swept away *from* the sun. The same sun which attracts the nucleus seems to repel the emitted matter with inconceivable energy. Consider for a moment what took place with Newton's comet in 1680–81 (fig. 4). When this comet was about as far off from the sun as our earth (ninety million miles) it began to throw out a tail. But the comet was going far nearer to the sun than this. Onwards it rushed under the powerful influence of the sun's attraction, until it had crossed the whole space of ninety million miles, making —almost in a straight line—for a point only one hundred and thirty thousand miles from the sun's surface. In four weeks it traversed that vast distance, and then, suddenly (in a few hours) sweeping half round the sun, started on its return journey. But note this : as it approached the sun, the comet had thrown out a tail continually increasing in length, and pointing *back* almost along the orbit ; then the comet is lost to sight for a few days, and when it is next seen returning rapidly from the sun, it has a tail pointing *forwards* (a tail which *must* be a different one, since—as Herschel says—'we cannot conceive a comet's tail to be brandished round like a stick') and ninety million miles in length. So that, whereas the comet, already moving with a tremendous acquired velocity, had taken four weeks in traversing a distance of ninety millions of miles under the sun's *attraction*, the matter composing the tail had been thrown to the same enormous distance by the sun's *repulsion* in scarcely one-tenth part of the time, possibly (for the tail was formed when first seen) in a few hours !

The comet of 1843 (fig. 5), was yet more remarkable for the dimensions of its tail and for its close approach to the sun. The tail of this comet stretched half-way across the sky in March, 1843. Its real length was two hundred million miles *at least*, for the end of the tail was lost to view

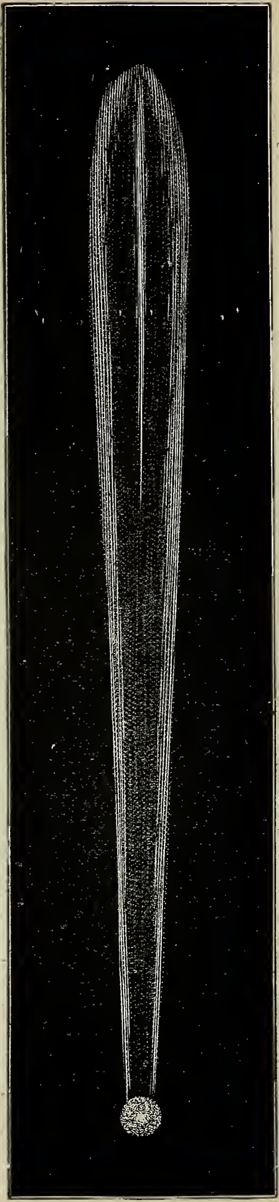

Fig. 4.—Comet of 1680 (*Newton's*).

through the excessive faintness of its light. So near did this comet pass to the sun, that many astronomers did not expect ever to see the comet again. But—after all but grazing the sun—sweeping round him at a distance of less than one-tenth of his diameter, the comet escaped and passed back again into space.

When we see the tail of a comet occupying a volume thousands of times greater than that of the sun itself, the question naturally suggests itself, 'How does it happen that so vast a body can sweep through the solar system without deranging the motion of every planet?' Conceding even an extreme tenuity to the substance composing so vast a volume, one would still expect its mass to be tremendous. For instance, if we supposed the whole mass of the tail of the comet of 1843 to consist of hydrogen gas (the lightest substance known to us), yet even then the mass of the tail would have largely exceeded that of the sun. Every planet would have been dragged from its orbit by so vast a mass passing so near. We know, on the contrary, that no such effects were produced. The length of our year did not change by a single second, showing that our earth had been neither hastened nor retarded in its steady motion round the sun. Thus we are forced to admit that the actual substance of the comet was inconceivably rare. A jar-full of air would probably have outweighed hundreds of cubic miles of that vast appendage which blazed across our skies, to the terror of the ignorant and superstitious.

The dread of the possible evils which might accrue from encountering a comet, will be diminished by a consideration of the extreme tenuity of these objects. But the feeling may still remain that influences, other than those due to mere weight or mass, might be exerted upon terrestrial races in the course of such an encounter. The subtle breath of some mephitic vapour might penetrate our atmosphere, and, if it did not bring immediate destruction, might leave dire forms of plague and pestilence to work their evil will upon the human race. This fear is not, perhaps, wholly

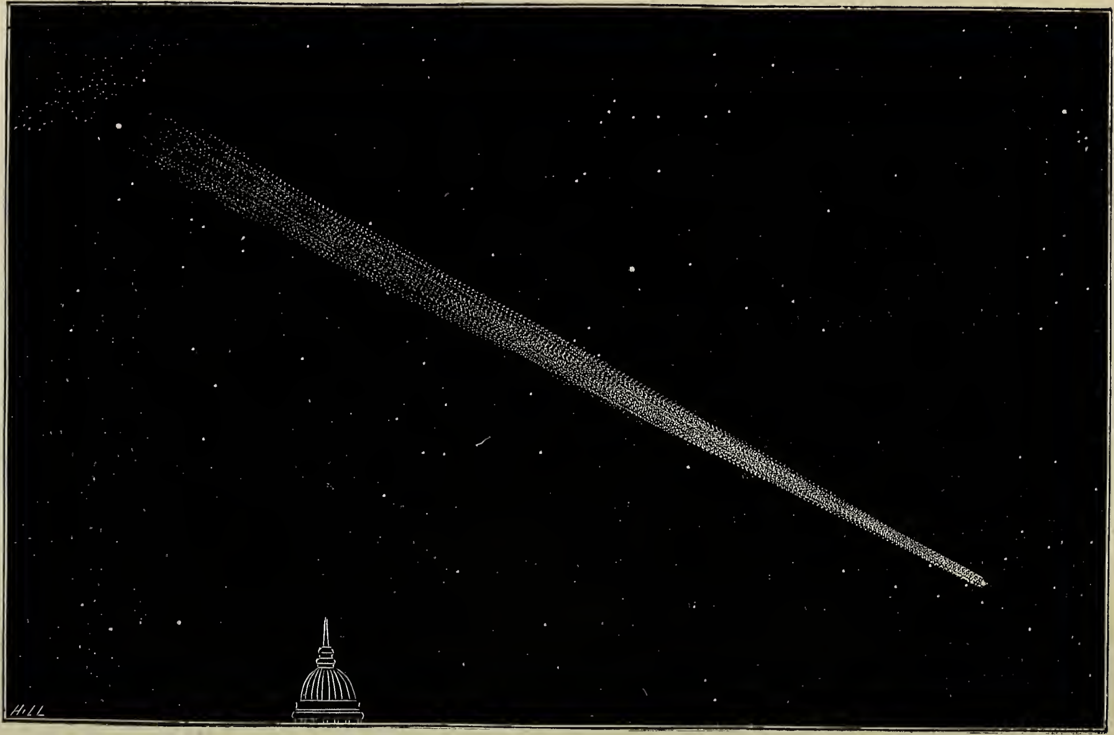

FIG. 5.—Comet of 1843.

unreasonable, though—as will presently appear—the positive information we now have does *not* favour the supposition that the tail, at any rate, of a comet is likely to exercise such destructive effects. And it is only the tails of comets that we have much chance of meeting. On account of their enormous volumes, it is not so utterly improbable that we should encounter them as that we should meet the comparatively minute nuclei. In fact, there is reason for supposing that the earth actually did pass through the tail of the comet of 1861. At about the hour when it was calculated that the encounter should have taken place, a strange *auroral glare* was seen in the atmosphere, but beyond this no effect was perceptible.

From what we have already seen, it will be manifest that the formation of comets' tails is a process of a very marvellous nature, apparently involving forces other than those with which we are acquainted. The tail, ninety millions of miles in length, which was seen stretching from the head of Newton's comet nearly along the path which the retreating comet had to traverse (the comet thus passing away with its tail in front, instead of behind, as when it approached the sun), must, it would seem, have been formed by some force far more active than the force of gravity. The distance traversed by the comet in the last four weeks of its approach to the sun under gravity was no greater than that over which the matter of the tail, seen after the comet had circled around the sun, had been carried in a few hours. Yet we have no other evidence of any repulsive force at all being exerted by the sun—at least, no evidence which can be regarded as demonstrative—and still less have we any evidence of a repulsive force exceeding in energy the sun's attracting power.

This difficulty, and the circumstance that a comet's tail lies in the direction opposite to the sun, or in the position which the shadow of the head would occupy, has led many, unfamiliar with the laws of optics, to suppose that the comet's tail may be simply the track of the luminous rays which have passed through the comet's head. They seem to think that the head may act in some way to send a beam of condensed light along the region opposite to the sun. It should hardly be necessary, however, to explain that no such beam of light could ever be seen where we see the comet's tail. The cases supposed to correspond with the formation in this way of the tail-like appendage are, in reality, of an entirely different kind. Thus, when we see a long beam extending from a bright light, we find that first the light has been caused to pass in that direction only (as when light is admitted into an otherwise darkened room through a hole); and secondly, there is matter along the course of the light to be illuminated. The beam is simply that long array of material particles which the light illuminates while leaving the particles in neighbouring space in darkness. So understood, such a beam is seen to be utterly unlike a comet's tail; for, in the first place, we know of no matter behind the head to be illuminated; and in the second, we know that light is falling on the regions all around the apparent array of illuminated particles, so that these surrounding regions should be as brightly lit up, which is not the case.

If any further doubt could remain as to this theory, it would be removed by, first, the circumstance that the tail of a comet is generally curved; and, secondly, the existence of several tails extending from the head of one and the same comet.

Professor Tyndall started a theory based on physical experiments, and otherwise in better accordance with scientific possibilities. Having found that certain gases, even in an exceedingly attenuated state, form a luminous cloud under the action of the electric light, he suggested that a comet's tail may be a luminous cloud of this sort, formed in the æther of space by those rays of sunlight which have passed through the comet's head. The rays which, without passing through the head, fall on the æther of space, would not call into existence this visible cloud, because their heating action would destroy what their chemical or actinic action *by itself* would produce; and as fast as the cloud behind the head came, through the comet's motion, under full solar action, it would be destroyed. So the tail would always be behind the head.

It appeared to Professor Tyndall that the curvature of a comet's tail, or the existence of more tails than one, as in Donati's Comet (figs. 6 and 7), was not inconsistent with this interpretation. For he noticed that, according to the gas dealt with, the luminous cloud would take a longer or shorter time in becoming visible. And he suggested that when the cloud formed slowly, the tail would be curved, the part near the head being behind the position which the head had recently passed through, while the part near the end of the tail would be behind the regions through which the comet had passed much earlier. Such luminous trails as were formed more quickly would account, he considered, for the straighter tails. He overlooked, I think, the circumstance that the shape of the luminous cloud-trail would not in reality depend at all upon the length of time which the cloud might take in becoming visible. Light would pass with the same velocity through the different kinds of tenuous gas, and whether the cloud became visible at once along the space thus passed through, or did not become visible for several seconds, or minutes, or even hours, it would become visible at the farther end of its course only just so long after it had become visible at the nearer end, as light had taken in traversing the length of cloud so formed. This interval of time would be the same for the quickly-appearing as for the slowly-appearing luminous cloud, and there would, therefore, be no difference between their forms. It would be necessary to account in this way for the curvature of the larger tail in the figure, as compared with the straightness of the smaller tails, that the curved tail should have been more slowly extended from the head; whereas the theory gives the same rate of extension for both, namely, the rate at which light travels.

We seem almost forced, by the phenomena of such a comet as Donati's, to adopt the theory of the actual repulsion of matter from the head of the comet into the tails—matter repelled most swiftly forming the straighter tails, while matter repelled more slowly, and seemingly in greater abundance, forms the great curved tail.

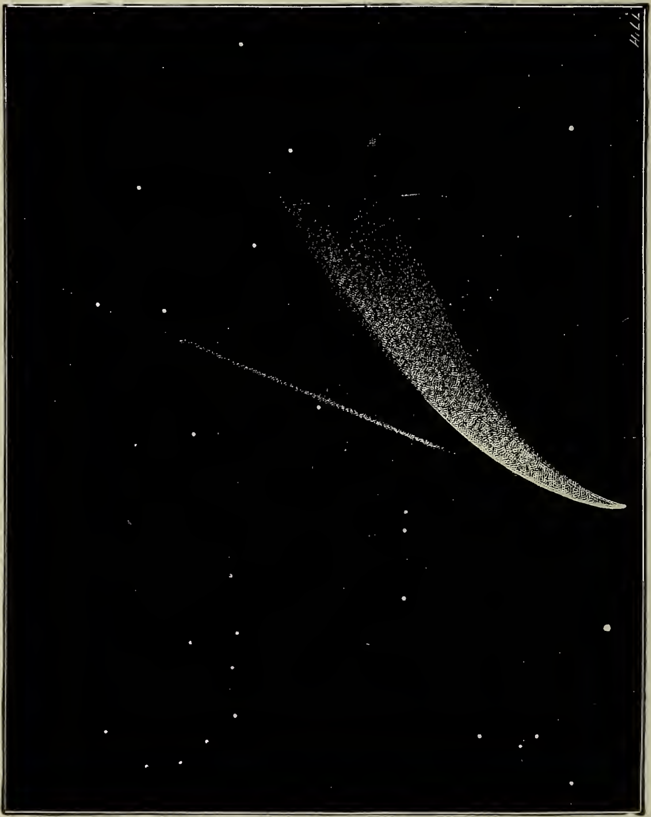

Fig. 6.—Donati's Comet, September 24, 1858.

We shall proceed to consider further on the evidence which seems to show that, strange though this theory of material repulsion may be, it is in point of fact the only admissible theory. If this shall be established, we shall have to admit the existence of a repulsive force, whose action on the grosser material of planetary bodies is insensible.

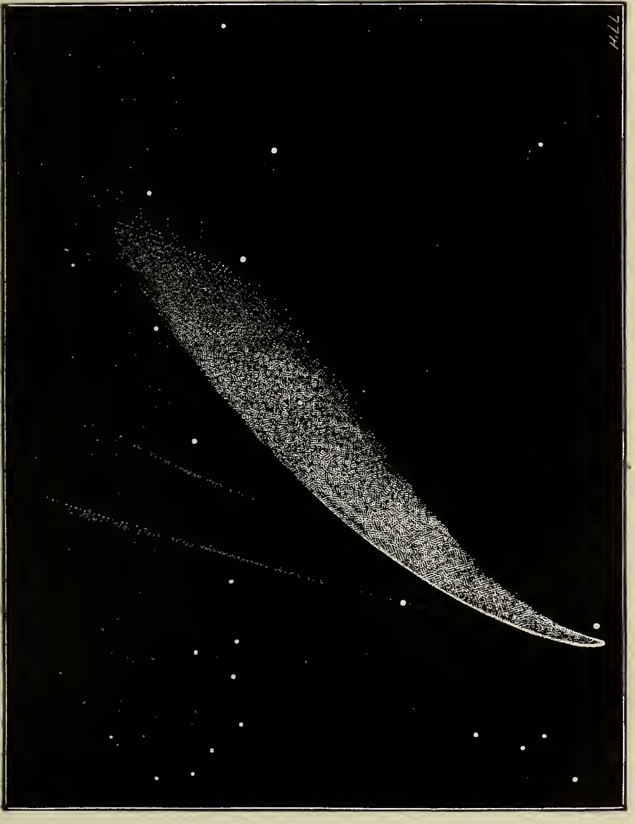

FIG. 7.—Donati's Comet, September 26, 1858.

Before we proceed to consider the theory by which, so far as can be judged at present, the phenomena of comets' tails can be explained, it may be well that we should consider the evidence derived from other comets than those hitherto considered.

In the first place we would direct special attention to the comet of 1811. In this comet, as may be seen from its picture in fig. 8, the various parts of the comet and its tail could be distinguished by the naked eye. There was the condensed part, called the *nucleus*, which in this case was apparently globular in form; the nebulous envelope which surrounds the nucleus, the so-called *coma*; the bright side-parts of the tail where it seems to be swept away from the coma, leaving a comparatively dark region behind the head; and the tail, widening and growing fainter with distance from the head. No one, we think, who considers this picture will for a moment imagine that the comet is a mere lens, and its tail merely the track of light condensed by this lens along the region behind the head. Here, again, the hollow structure of the tail seems indicated by the bright tracks on either side, though, as we shall endeavour to show later, the exceedingly well-defined nature of the dark track behind the nucleus in many comets seems to force upon us a different interpretation of this singular and characteristic feature.

In some respects the comet of 1811 tells us more of cometic possibilities, so to speak, than any other comet that has ever yet been observed. Discovered on March 26, 1811, this comet remained visible for a longer time than any yet seen, viz. for 16 months, 22 days. It had a tail 120 millions of miles in length, and 15 millions of miles in diameter at the widest part. The diameter of the nucleus was about 127,000 miles; that of the envelope round the head about 643,000 miles. But what was so remarkable about this comet was, that it obtained this remarkable development without approaching the sun, as other comets have done. The usual rule with comets is that the nearer they approach to the sun, the more their head and tails are developed. But the least distance of the comet of 1811 from the sun was little less than 100 millions of miles. Again, although it had so remarkable an appearance, as seen from the earth, the distance of that comet from us was at no time less than 110 millions of miles. Its true magnitude, therefore, as Professor Kirkwood well remarks, 'has probably not been surpassed by that of any other comet which has yet been observed.' If its path had carried it nearer to the sun, its appearance would probably have been terrible in the extreme. If we consider the enormous volume occupied by this comet and its tail, its

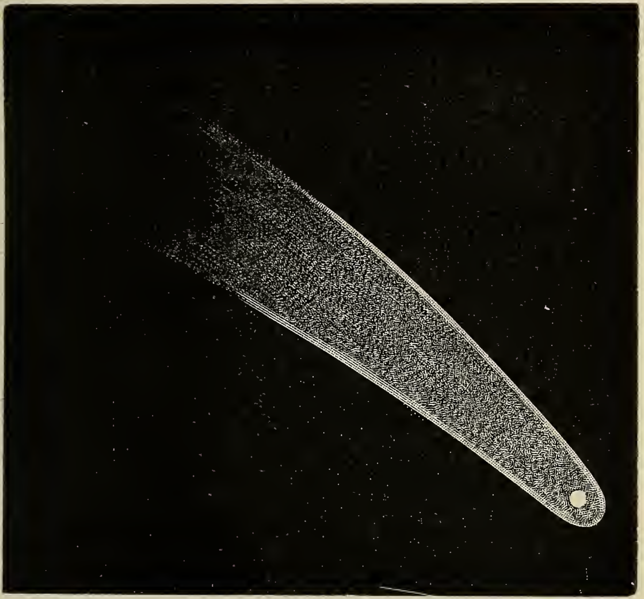

FIG. 8.—Comet of 1811.

Million cubic miles of head,
Ten billion leagues of tail,

we shall see that the phenomena we have to interpret ought not to escape us because of minuteness of scale.

Next consider the great comet of 1861. This comet was discovered on May 13, by Mr. John Tebbutt, jun., of New South Wales, and first accurately observed at the Sydney Observatory, on May 26. It passed northwards from the southern skies, and first became visible in Europe in the last week of June, 1861. The first recorded observations were made on the evening of June 30, nineteen days after it had passed its point of nearest approach to the sun. I remember well observing it on the morning of July 2, 1861. For some reason, I found it impossible to sleep that morning, and getting up about three in the morning (the exact hour I do not remember, but it must have been very early), I saw in the east what looked at first like the rays of an aurora borealis. But presently I noticed that these rays proceeded (unlike those of the aurora) from a bright centre, which had been hidden by clouds when my observations began. I used at that time to keep a four-inch telescope, mounted on a three-legged stand, in my bedroom. This I had quickly ready for action (noting that the object, owing to the approach of sunrise, was getting fainter every minute), and turning it on the comet, I drew a picture of the nucleus and coma so closely resembling that which appeared a week or two later in the 'Illustrated London News,' that I might have supposed my picture had been surreptitiously sent to the office of the 'Illustrated,' had I not found it resting just where I had put it in my scientific portfolio.

The comet appeared to the eye as shown in fig. 9. Sir John Herschel, who observed it at Collingwood, in Kent, remarked that it was far more brilliant than any comet he had ever seen, not even excepting those of 1811 and 1858. The Padre Secchi, at Rome, found that in the clear skies of Italy the tail was fully 118° in length, corresponding to nearly one-third more than the distance between the horizon and the point overhead. This comet, by the way, though only favourably visible for a very short time, remained within the range of telescopic vision much longer. Hind remarks that the number of separate observations for the determination of its orbit exceeded 1,150, and extended over a period of 11½ months. It travelled on a course favouring observation, coming from remote distances south of the plane in which the earth travels to the northern side of that plane—and, as it chanced, crossing the plane (about five-sixths of the way from the sun to the earth's orbit) just when the earth lay in the same direction from the sun, so that for a time she was within the bounds of the comet's tail-like appendage—and then travelling northwards on a path almost at right angles

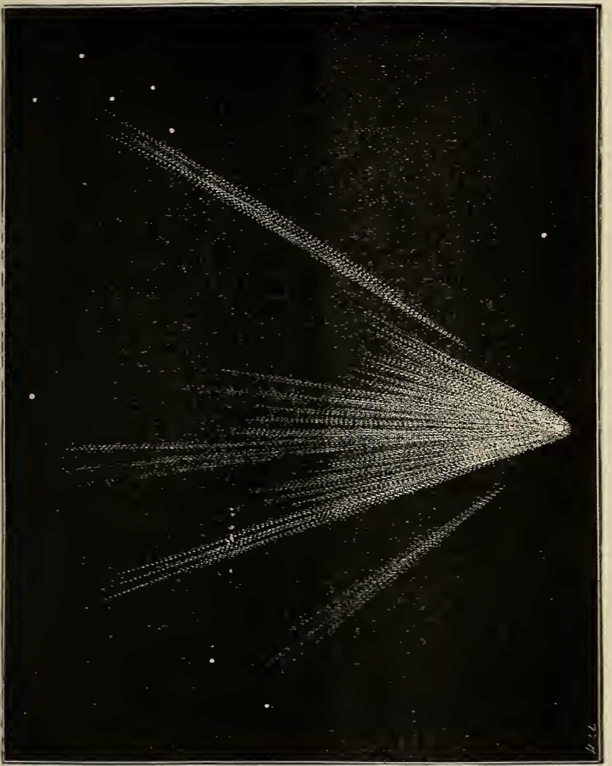

Fig. 9.

to the plane of the ecliptic. Thus the comet could be tracked on its retreat until, finally, distance concealed it from our view.

Now, the tail of the comet of 1861, as seen in fig. 9, had something of the fan-like expansion observed in the tail of the comet of 1744; but what was known of the comet's position at the time when this fan-like form was seen, explained the peculiarity, and showed the necessity of taking into account the position of a comet before attaching undue importance to the apparent figure of its tail. For the fan-like form seen on this occasion was a mere effect of perspective. The end of the tail appeared very much wider than the part near the head—not that it really was so, but simply because it was very much nearer to the observer on earth. When we were actually immersed in the tail, the part nearest to us, being all round, had, to all intents and purposes, an infinite extension. But even when the comet was beyond that position, or a few days earlier, before it had reached it, the end of the tail was much nearer to us than the comet's head, and thus appeared far more proportionately widened than was actually the case.

Such considerations must always be taken into account in dealing with cometic phenomena. Comets, more than any other celestial objects (the Milky Way, regarded as a whole, being, perhaps, alone excepted), are affected in shape, and (apparently) even in their very nature, by position, and consequent fore-shortening.

It may be well here to consider a case in which some active force (other than gravity), exerted by the sun, seems to have wrought the destruction of a comet, or at least to have broken up the comet into unrecognisable fragments.

No comet ever observed has exhibited phenomena more remarkable than those displayed by the comet known as Biela's (more properly called Gambart's). I wish I could agree with a modern astronomer, who has said that no comet has thrown more light on the nature of these bodies; but, in point of fact, it is only promise of light, not light itself, that we have obtained.

Discovered in 1826, Biela's comet was presently found to be identical with one seen in 1772 by Montaigne, and again by Pons in 1805. A careful study of the observations showed that the comet travels round the sun in a period of about 6⅔ years, or, roughly, thrice in twenty years. Its path was found to approach very near to the path of our earth. The comet returned in 1832, when the ignorant were scared much as they have been recently by the threatened influence of the larger planets in perihelion. The comet crossed the earth's track several weeks before she herself came to the place where the two orbits approach nearest, and it is hardly necessary to say that the comet's passage did not injure the earth's roadway in any appreciable degree.

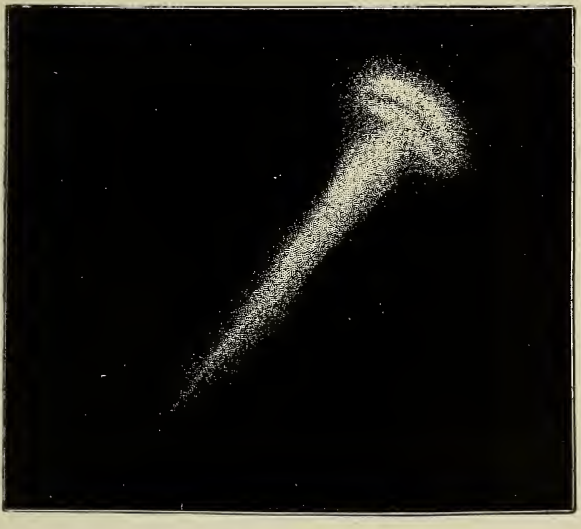

FIG. 10.—Biela's Comet in 1846, before its division into two.

In 1839 the comet returned, but was not seen, travelling across a part of the heavens only above the horizon in the day-time, so that the comet's light was hidden by the sun's.

It was at the next return in 1845–46 that the comet first attracted special attention. On that occasion, instead of behaving as comets usually do, Biela's, which in the first days of 1846 had presented the appearance shown in fig. 10, was found to have divided into two. There is some little doubt as to the time when the comet underwent division. Lieut. Maury reported on January 15 that he had seen the comet double on January 13; but Wichmann observed it as a single comet on the 16th. But Professor Challis, in his account of his own observations on the comets, states that even on January 15 the second comet might easily have been overlooked. M. Valz saw nothing unusual on the 18th and 20th; but on the 27th: 'I was struck with amazement,' he says, 'to find two nebulosities, separated by an interval of two minutes of arc, instead of one nebulosity alone. . . . Each head was followed by a short tail, whose direction was perpendicular to the line joining the two nebulosities.' Earlier, only the larger comet had had a tail, the appearance presented by the double comet being that shown in fig. 11.

The two comets travelled along, side by side, until at last both passed out of view, at which time the distance between them amounted to about 157,000 miles.

In 1852 both comets returned. Sir John Herschel says, in his 'Familiar Lectures on Scientific Subjects,' that when they returned, the distance between them was unchanged. This, however, was a mistake. The distance now amounted to about 1¼ millions of miles. Again they passed before the interested gaze of astronomers, travelling side by side, though rather far apart, until finally they disappeared from view—I say finally, for neither has ever been seen again.

Whether the two comets returned in 1859 is doubtful. It is certain that, if they did, they would have been invisible, for the same reason that the comet was invisible when it returned in 1839.

But in 1866 the double comet should have been well seen. It should be remembered that each return of a comet of short period (like that which our correspondent Mr. F. Denning, of Bristol, discovered this year) gives the astronomer more perfect mastery of the comet's motions. The return could be predicted with sufficient accuracy in 1832 to cause the comet to be easily redetected. The next visible return might have involved a difficulty, because the comet had in the interval made two circuits. But that return was successfully predicted. The return in 1845–46 was still more accurately calculated. Nor did the breaking up of the comet into two on that occasion interfere with the successful calculation of the return in 1852. The case may be compared to the rating of a clock, which is more satisfactorily effected in a week than in a day, for the simple reason that any error of observation is spread in one case over seven times as long a period as in the other, and therefore affects

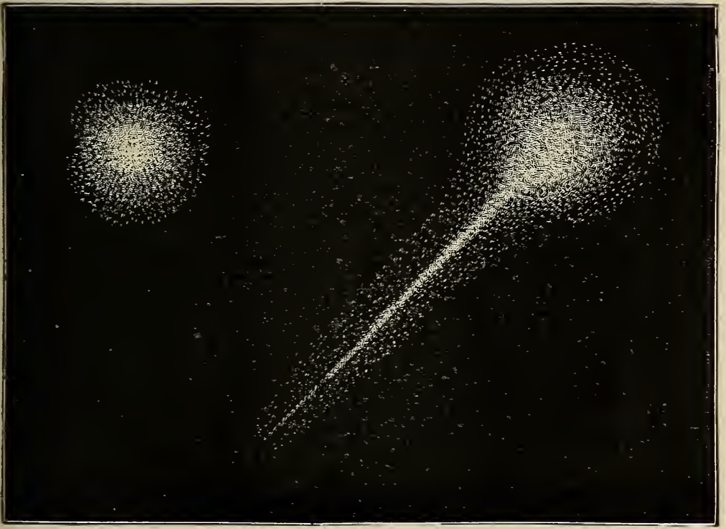

FIG. 11.—Biela's Comet on January 15, after its division into two.

the estimate of any given circuit of the hands by an error only one-seventh as large. Just so, whatever error an astronomer might make in observing Biela's comet in, say, 1843, was distributed over all the revolutions of the comet which had taken place since 1826 (one might almost say since 1772), and in a correspondingly small degree affected the astronomer's estimates of the comet's motion during any single revolution. This being so, astronomers had good reason for believing that in 1866 Biela's comet would return. When the time came that it should have been visible, telescopes were turned towards the spot where it should have been seen. Night after night from that time its calculated track was swept with the finest telescopes in Europe and America. But no trace of the comet could be seen. 'It is now,' wrote Sir John Herschel, in February, 1866, 'overdue. Its orbit has been recomputed, and an ephemeris calculated. Astronomers have been eagerly looking out for its reappearance for the last two months, when, according to all former experience, it ought to have been conspicuously visible, but without success! giving rise to the strangest surmises. At all events, it seems to have fairly disappeared, and that without any such excuse as in the case of Lexell's—the preponderant attraction of some great planet. Can it have come into contact, or exceedingly close approach to some asteroid as yet undiscovered; or, peradventure, plunged into and got bewildered among the ring of meteorolites, which astronomers more than suspect?'

Be the cause what it might, the comet was not seen in 1866. In 1872 it was looked for even more carefully. Every possible contingency depending on planetary perturbations was considered; and the telescopes of astronomers swept, not only the calculated path, but to a considerable distance on either side of it. No trace of the comet was seen, however, in 1872 any more than in 1869. So far as telescopic observation is concerned, Biela's comet seems to have come to the end of its career as a comet.

Yet the observations of 1852 were not the last which were made on this interesting object. It has been seen again, though not as a comet. Nay, the occasion on which it was seen in the way referred to was predicted, and the prediction fulfilled, even in details. For a full account of its reappearance—as a meteor stream—I refer the reader to my essay on Biela's Comet in 'Familiar Science Studies.'

COMETIC MYSTERIES.

During the last two years several comets—some telescopic, others visible to the naked eye, and even conspicuous objects in the heavens—have been observed, not only by the older methods, but by some which have only been available within recent years. It is naturally expected, therefore, by the general public that some new light should be thrown on these mysterious objects, whose phenomena still remain among the unexplained, seemingly the inexplicable, problems of the celestial depths.

We propose to consider here what has thus been learned, and what also (unfortunately it is much more) remains still to be learned, respecting comets. But first it will be well to show what are the special phenomena which present themselves for explanation.

A comet apparently comes out from the remote depths of space in a condition of comparative calm. It appears as a small round nebulous object, looking like a tiny cloud of extreme tenuity—the idea of tenuity being suggested by the exceeding faintness of the comet's light. This cloud appears somewhat condensed towards the middle. As the comet draws nearer to the sun, it usually grows somewhat long in the direction of the sun; and before long a portion within the part nearest the sun is seen to be brighter than the rest, and to have a more or less defined outline. This is the *nucleus*—sometimes seen as a larger dull disc of nearly uniform brightness, at others as a mere bright point, not unlike a star. The fainter light around this is the *coma*, or hair, which resembles a luminous fog round the nucleus, usually brighter on the side towards the sun, and on the other side growing fainter and fainter till it can no longer be seen. Later, this lengthening of the comet in directions towards and from the sun becomes more marked, until at length the comet may fairly be said to have a head directed towards the sun and a tail directed from him. Nucleus, coma, and tail may be very different in appearance in different comets, and in particular the tail may be more or less complicated in structure, being sometimes a mere straight streak, at others twofold, multiple, curved, with thwart streaks, and so forth—no two comets, in fine, having tails resembling each other except in general details.

Dr. Huggins, in a rather disappointing article on comets, recently communicated to the *Nineteenth Century*, remarks that the nucleus, though an apparently insignificant speck, 'is truly the heart and kernel of the whole thing—potentially it is the comet.' This has scarcely yet been proved, though it appears exceedingly probable. It is true, however, as he adds, that this part only of the comet conforms rigorously to the law of gravitation, and moves strictly in its orbit. 'If we could see a great comet,' he proceeds, 'during its distant wanderings, when it has put off the gala trappings of perihelion excitement, it would appear as a very sober object, and consist of little more than nucleus alone.' This again seems probable, though it has never yet been proved, and the division of some comets into two or more parts, each having coma, nucleus, and tail of its own, shows that the nucleus cannot be, in every case, what Dr. Huggins seems here to suggest. Dr. Huggins has done well in saying (though scarcely with sufficient emphasis, considering how often the mistake is repeated) that 'though many telescopic comets are of extremely small mass, nucleus included—so small, indeed, that they are unable to perturb such small bodies as Jupiter's satellites—yet we should mistake greatly if we were to suppose that all comets are "airy nothings." In some large comets the nucleus may be a few hundred miles in diameter, or even very much larger, and may consist of solid matter. It is not necessary to say that the collision of a cometary nucleus of this order with the earth would produce destruction on a wide scale.'

It is even more necessary to correct the widely-spread misapprehension as to the relations between meteors and comets. We hear it stated that the nucleus of a comet is made up of meteoric stones (Professor P. G. Tait says—for unknown reasons—that they resemble 'paving stones or even bricks') as confidently as though the earth had at some time passed through the nucleus of a comet, and some of our streets were now paved with stones which had fallen to earth on such an occasion. As a matter of fact, all that has yet been proved is that meteoric bodies follow in the track (which is very different from the tail) of some known comets, and that probably all comets are followed by trains of meteors. These may have come out of the head or nucleus in some way as yet unexplained; but it is by no means certain that they have done so, and is by many astronomers regarded as more than doubtful.

The most important points to be noticed in the behaviour of large comets, as they approach the sun, is that usually the side of the coma which lies towards the sun is the scene of intense disturbance. Streams of luminous matter seem to rise continually towards the sun, attaining a certain distance from the head, when, assuming a cloud-like appearance, they seem to form an envelope around the nucleus. This envelope gradually increases its distance from the sun, growing fainter and larger, while within it the process is repeated, and a new envelope is formed. This in turn ascends from the nucleus, expanding as it does so, while within it a new envelope is formed. Meanwhile, the one first formed has grown fainter, perhaps has disappeared. But sometimes the process goes on so rapidly (a day or two sufficing for the formation of a complete new envelope) that several envelopes will be seen at the same time, the outermost faintest, the innermost most irregular in shape and most varied in brightness, while the envelope or envelopes between are the best developed and most regular.

The matter raised up in these envelopes seems to have undergone a certain change of character, causing it no longer to obey the sun's attractive influence, but to experience a strong repulsive action from him, whereby it is apparently swept away with great rapidity to form the tail. 'It flows past the nucleus,' says Dr. Huggins, 'on all sides, still ever expanding and shooting backwards until a tail is formed in a direction opposite to the sun. This tail is usually curved, though sometimes rays or extra tails sensibly straight are also seen.' The description is, however, incomplete in one important respect. The matter raised from the nucleus to form the envelopes may be, and probably is, carried past the nucleus *on all sides*; but the appearance presented by the tail just behind the nucleus is not exactly in accordance with our ideas as to what should result from the flowing past 'on all sides.' There is a dark space immediately behind the nucleus, that is, where the nucleus, if solid, would throw its shadow, if there were matter to receive the light all round so that the shadow could be seen. Now it may be thought at first that this corresponds exactly with what should be seen : when we look just behind the nucleus there is no light or very little ; when we look on either side of that dark space there is the luminous matter which has been driven back from the envelopes in front of the nucleus. But if the luminous matter flows past the nucleus on all sides, it must flow past the nucleus on the side nearest to the observer, and also on the side farthest away; and it is just where the line of sight passes through these two regions of brightness that a dark streak is seen behind the nucleus. Let the reader draw two concentric circles—one an inch in diameter, the other two inches—and let him then draw two parallel tangents to the inner circle on opposite sides of it. Supposing now the space between the two circles to represent in section the luminous matter which flows all round the nucleus, while the surface of the inner circle represents the unilluminated part behind the nucleus, the two tangent lines will represent the lines of sight on either side of the dark region, where, as we might expect, we get plenty of light; and we can also understand very well why outside of that the line of sight through the luminous matter (or the chords to our outer circle), getting shorter and shorter, the light of the luminous streaks bounding this part of the tail gets fainter and fainter: but if just inside either of the two tangents, chords are drawn parallel to them, crossing the inner circle, the parts of these chords which lie between the two circles are very nearly equal in length to the tangent lines themselves; and even a common diameter to both circles has, lying between them, two portions together equal to the radius of the outer. Hence, since the line of sight even across the middle of the space behind the nucleus, passes through a considerable range of luminous matter, while a line within but near the outskirts of that space passes through nearly as great a range of luminous matter as one just outside that space, there should be plenty of light where yet to the eye there seems to be something like absolute darkness. Either then the eye is greatly deceived, or else we must find some explanation of darkness existing where considerable brightness might be expected.[1]

The matter which forms the tail, seems, as I have said, to be swept off from the envelopes raised by the sun's action on the nucleus. It seems as though the matter thus raised had undergone in some way a change of character, which caused it no longer to obey the law of gravity as it had done when forming part of the nucleus, but instead of yielding to the sun's attraction to submit rather to an intense repulsive action, carrying it at a much greater rate from the sun than, under the action of gravity—starting from rest and free from all perturbing influences—it could have been drawn towards him. Dr. Huggins thus words his account of what seems to happen : 'Now is seen to take place a change which is most puzzling—namely, these envelopes of light appear to give up their substance under the influence of a strong repulsive force exerted from the sun, and to be forced backwards.' Sir John Herschel, after his long and careful study of the comet of 1830 (Halley's at its second return) came to the conclusion that repulsive action exerted by the sun on the matter raised in these envelopes had been distinctly proved.

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[1] Should the careful examination of satisfactory photographs seem to show that the darkness (almost blackness) behind the nucleus is an objective and not merely a subjective phenomenon, the following explanation would seem forced upon us. If the particles forming the envelopes are minute flat bodies, and if anything in the circumstances under which these particles are driven off into the tail causes them to always so arrange themselves that the planes in which they severally lie pass through the axis of the tail (which, if the tail is an electrical phenomenon, might very well happen) then we should find the region behind the nucleus very dark or almost black, for the particles in the direction of the line of sight there would be turned edgewise towards us, whereas those on either side or in the prolongation of the envelopes would turn their faces towards the observer.

Yet here, where we seem to have our first firm ground for hypothesis respecting these mysterious objects—comets' tails—we meet with stupendous difficulties. Consider, for instance, the phenomena presented by Newton's comet, That comet had traversed the last 90 millions of miles of its approach towards the sun in four weeks. At the end of that time it passed out of view for a few days, having then a tail 90 millions of miles, at least, in length. Four days passed, and it reappeared on the other side of the sun—having in the interval traversed nearly a semi-circle—in reality, of course, the perihelion end of its long oval path. At its reappearance, it had a tail still 90 millions of miles in length, but the tail with which it reappeared had, of course, a direction entirely different from that of the tail which had been seen before—the two directions were inclined about 160 degrees to each other. Now, as Sir John Herschel remarks, we cannot look on the tail of a comet as something whirled round like a stick, as the comet circles around its perihelion sweep. The tail with which the comet reappeared must have been an entirely new formation. Nor can we doubt that if the comet could have been watched as it swept around the sun, the changes in the tail's position which had been observed to the time of disappearance, would have been found to progress continuously, the tail passing by a uniform motion from the position it then had to that which it was observed to have at the time of reappearance. So that we may fairly suppose the tail with which the comet reappeared to have been formed in much less than the time during which the comet had been out of sight. Probably its farthest part had been formed in much less than a day, the part near the head being, of course, formed later. But if the matter repelled from the head was thus driven over a distance of 90 million miles in twenty-four hours, at the outside, the average velocity of its motion was about a thousand miles per second, or nearly three times as great as the greatest velocity which the sun can communicate by his attractive energy to matter approaching him from without, even though such matter come to him from an almost infinite distance, and in a perfectly straight line—the conditions most favourable for giving a high rate of final velocity. Such velocity as the sun *can* thus give by his attractive energy is only given to matter which has been exposed a long time to his influence : but here, in the tail of the great comet of 1680, matter seems to have acquired almost instantaneously a velocity sufficing to carry it over 90,000,000 miles with an average speed three times as great as the sun can thus, after long effort, communicate by means of his attractive power !

The difficulty is so great that many efforts—some bold and daring, others positively wild in the unscientific absurdity of their nature—have been made to overcome it.

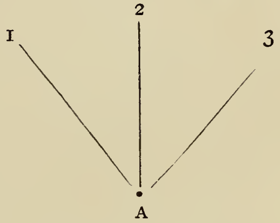
Among the most ingenious of these is (or rather was, for I think it is no longer maintained even by its eminent author) Professor Tyndall's theory of a comet's tail as an actinic cloud, generated by the passage of the solar rays through exceedingly tenuous matter, after those rays had been in part deprived of their heating power during their passage through the comet's head. According to this theory the actinic cloud cannot be formed under the heating rays, but so soon at the actinic rays fall on the tenuous matter alone, the cloud is formed,—so that all round the region in which would be the comet's shadow, there is no luminous cloud, while along that region the cloud exists. The rapidity with which light travels would of course make this explanation absolutely perfect in explaining cometic tails lying always exactly in a straight line directed from the sun, or with their axis so situated. But unfortunately this exceedingly rapid formation of the tail (a tail of 90 milion miles in length would be formed in about eight minutes) is more than observation requires or can explain. Professor Tyndall made a slight oversight in dealing with this part of his theory. Noticing that the actinic cloud, as he called it, is not formed instantly, but after a delay of a few seconds, in his experiments, he reasoned as though it would follow from this that the formation of the actinic cloud behind a comet's head in space might be a process extending its action in distance from the head at a rate considerably less than that at which light travels, yet still fast enough to account for the exceedingly rapid formation of the tail of Newton's comet, and of other similar tails. But a little consideration will show that the few seconds following the fall of light on the vapours dealt with by Tyndall, before the luminous cloud appeared, would produce no such effect as he imagined. The rate of formation of the tail would still be that at which light travels. Imagine the head at A, for the sake of argument, and the sun's light after reaching A, passing on to B, C, D, E, &c., to Z, a distance say of 100,000,000 miles, in nine minutes.

A . . . B . . . . C . . . . D . . . . E . . . . Z.

Suppose that, when the light has reached the vaporous matter lying at B, an interval of one full minute (much greater than any noticed in Tyndall's experiments) occurs before the actinic cloud comes into view, a similar interval after the light has passed C before the cloud is seen there, and so on, up to the time of the arrival of the light at Z. Professor Tyndall's reasoning implied that all the time-intervals thus occurring at B, C, D, E, &c., up to Z, had to be added together, to give the total time of the formation of the tail from A to Z; and hence naturally a long time might elapse, and the head having at the end of this time reached a different position from that which it had occupied at the beginning, the divergence of the tail from the direction exactly opposite to the sun, and the curvature of the tail, would be alike readily accounted for. But what are the actual facts of the case? The part of the tail formed latest by the supposed solar actinic action, namely, the part at Z, would be formed just ten minutes after the light had left A, and nine minutes after the part nearest to A had been formed (by the same light waves), for, nine minutes after leaving A, the light would be at Z, and a minute after such epoch (according to our supposition) the actinic cloud would be formed respectively at A and at Z. We get just the same interval—nine minutes—whether the actinic cloud appears immediately after light has traversed the vapour which is to form the cloud, or a minute after, or an hour after. In every case the tail would be formed outwards from A, at the rate at which light travels. This does not accord with the phenomena,—in fact the supposition that a tail could be formed at the rate at which light travels, will be found, on examination, to lead to many most manifest absurdities, which Professor Tyndall doubtless recognised when he sought escape from the supposition of such rapid tail formation through the effects he attributed to the delayed appearance of the actinic cloud.

Another theory in explanation of the rapid formation of such a tail as that of Newton's comet is worthy of far less notice. Professor Tyndall's theory was based on an interesting physical fact, which he had himself discovered, and which was also manifestly akin in character to the formation of a comet's tail. The one to be now noticed was suggested to a mathematician by a rather familiar phenomenon, the effects of which on his imagination he seems to have been never able to entirely overcome,—at any rate no amount of evidence against the theory seems to counterbalance in his mind the notion once conceived that the theory might be true. (It is a way some theorists have.)

Professor Tait was once looking at a part of the sky which seemed clear. As he looked, a long streak rapidly formed, which presently disappeared (if I remember his original description aright) almost as rapidly as it had formed. At any rate, the appearance of the streak was rapid enough to remind him of what astronomers said about the rapid (apparent) development of comets' tails. The phenomenon itself was easily explained. There had been a flight of seabirds, travelling after their wont in a widely extended layer, which when he began his observations had been looked at somewhat aslant, so that—the distance being too great for the birds to be seen individually—nothing of the flight could be discerned at all. But it is evident that in such a case a very slight movement on the part of each bird would suffice so to shift the position of the layer in which they were travelling, that it would be seen edgewise, and then the birds, being so situated that the range of sight towards any part of the layer passed athwart a great number of them, would of course be seen, not individually but as a cloud, or long straight streak, a side view in fact of the layer in which they were travelling. *Eureka!* shouted Professor Tait; and presently announced to the world the marvellous theory that the rapid formation of comets' tails may be accounted for on the same general principle. Astronomers have found that along the tracks of some comets (where the tails never lie, by the way, but that is a detail) are countless millions of meteoric bodies separately undiscernible (and never yet discerned as a cloud—another detail); therefore it follows that the tails of all comets are formed by movements of 'brickbats and paving stones' in them (Professor Tait's own description of meteors), after the manner of the seabirds he saw from Arthur's Seat. Professor Thomson at the Edinburgh meeting of the British Association endorsed this theory with special reference to the value of the 'seabird analogy' in explaining the phenomena of Newton's comet. Dr. Huggins, who, as he does not claim to be a mathematician (or, to speak more correctly, as his labours in physical research have not given him time for profound mathematical studies), may be more readily excused, also speaks of this seabird theory as if it had some legitimate standing. 'The tail, he conceives,' he says, referring to Dr. Tait, 'to be a portion of the less dense part of the train illuminated by sunlight, and visible or invisible to us, according, not only to circumstances of density, illumination, and nearness, but also of tactic arrangement, as of a flock of birds under different conditions of perspective.' Of course, the theory is utterly untenable—by astronomers who know something of the actual facts, and have enough mathematics to consider simple geometrical relations. Bodies moving in a plane surface like birds, if they individually travel in the same plane, keep its position unchanged. But if they move individually at an angle to that plane (as they occasionally do), they change its position—the surface however in which they collectively are at any moment, still remaining plane. In such a case only could such a phenomenon as was observed by Professor Tait be seen. But in such a case the visibility of the streak formed by the flight of birds would last but a few minutes, for the same motion which had in a few minutes brought the streak into view would in the next few minutes take it out of view. During the short time that a flight is visible in this way, it has an unchanging position, or a scarcely changing one. If the tail of Newton's comet had rapidly formed and as rapidly vanished, remaining, while visible, in an almost unchanging position, the 'seabird analogy' might explain that particular phenomenon, however inadequate to explain multitudes of others. But the phenomena to be explained are entirely different. Leaving out of the question the varying position and length of the tail as it approached the sun, and after it left the sun's neighbourhood, all of which were entirely inconsistent with the seabird analogy, what we are called upon to explain is that a visible tail ninety millions of miles in length, seen in position 1A on one day, was seen three days later in position 3A (having manifestly in the meanwhile passed through all the intermediate positions, including 2A). If Professor Tait, profound mathematician though he be, though he may 'differentiate and integrate like Harlequin,' can show how any flight of bodies, like or unlike seabirds, can accomplish such a feat as the above, appearing first to form a thin streak A1, and in less than four days a thin streak A3, each 90 millions of miles long, without *some* of them having had to travel a distance nearly equal to the line from 1 to 3—or some 150 millions of miles long, instead of the trifling journeys he assigned them, he should take a rank above Newton and Laplace as a mathematician. But there is another feat apparently equally difficult to him, which he might achieve very readily with great advantage to those non-mathematicians among astronomers whom his name—well-deserved too—as a mathematician has hitherto misled, and with not less advantage to his own reputation: he might frankly admit that the idea which occurred to him while watching those unfortunate seabirds, had not quite the value which at the moment he mistakenly attached to it, and has since *seemed* to do.



But apart from the consideration of theories such as those, either demonstrably untenable, though ingenious, like Professor Tyndall's, or altogether and obviously untenable like Professor Tait's, there are certain phenomena of comet's tails which force upon us the belief that they are phenomena of repulsion, though the repulsive action is of a kind not yet known to physicists. Amongst these are :—

1. The curvature of all the cometic tails when not seen from a point in or near the place of their motion.

2. The existence of more tails than one to the same comet, the different tails being differently curved.

3. The phenomena of striations athwart the tail.

It is evident that all these phenomena are such as we might fairly expect if a comet's tail is caused by the sun's repulsive action on molecules, raised by his heating action on the head. The matter thus swept away would resemble smoke, driven upwards from the funnel of a moving steamer, and then swept in any given direction by a steady wind ; we should see a curved train of such matter just as we see a curved streak of smoke. If the matter raised from the head is not all of one kind (and it is antecedently unlikely that it should be), there would be more than one trail of matter, if the sun's repulsive action were different on these different kinds of matter. Lastly, the striations seen athwart the tail, as in the well-known case of Donati's great comet, would be explained, either as due to the observed pulsational manner in which the envelopes are raised (if matter were raised uniformly from the head there could be no formation of successive envelopes), or else as due to the carrying off into the main tail, where alone such striations are seen, of matter which, had it freed itself at the beginning, would have been swept off into the smaller tails, but being as it were entangled in the great outflow of matter forming the large tail, escapes later, and when it does, gets swept off at its own more rapid rate, and there forms a streak lying at an angle with the direction of the principal tail.

Bredichin has shown that where there are three tails to a comet, their forms correspond with the theory that the envelopes raised from the head are principally formed of hydrogen, carbon, and iron. But this, which, if established, would be the most important physical discovery yet made respecting comets, seems open at present to considerable doubt, though confirmation seems to be given to it, in some respects, by the results of spectroscopic analysis.

To spectroscopic analysis we must in all probability look for such information respecting comets as may hereafter enable us to understand their nature. On this point let us consider what is said by one who, if not the greatest living astronomical spectroscopist, is *facile princeps* in this country—Dr. W. Huggins. First, however, we must consider the past of this method of research as applied to comets.

The first successful application of the spectroscope to comets was made by Donati in 1864—the light of the comet being then divided into three bright bands, whose position, however, was not exactly determined. In 1866 Dr. Huggins obtained two kinds of light from a telescopic comet, part of the comet's light giving a continuous spectrum, probably reflected sunlight, the other a spectrum of three bands. In 1868 a comet was observed (Brorsen's) with more success. Three bands were seen in the spectrum of the light from the comet's head, and a comparison of these with measures of similar bright bands belonging to the spectra of various combinations of carbon, showed, or rather seemed to suggest, that 'combinations of carbon might be present in the comet.'

> In conjunction with my friend, the late Dr. W. Allen Miller (says Dr. Huggins) I confronted directly with the spectroscope attached to the telescope, the comet's light with that from inductive sparks passing in olefiant gas. The sensible identity of the two spectra left no doubt of the essential oneness of the cometary stuff with the gas composed of carbon and hydrogen that was employed for comparison. Since that time (proceeds Dr. Huggins) the light from some twenty comets has been examined by different observers. The general close agreement in all cases, notwithstanding some small divergences, of the bright bands in the cometary light with those seen in the spectra of hydrocarbons, justifies us fully in ascribing the original light of these comets to matter which contains carbon in combination with hydrogen.

Last year photography was applied to this spectroscopic work. The spectrum of the brightest comet of that year was partly continuous, and on this continuous spectrum many of the well-known Fraunhofer lines could be traced. This made it certain that part of the comet's light was reflected sunlight; though Dr. Huggins considers also that a part of the continuous spectrum of every comet is due to inherent light. On this point some doubt may be permitted. It is one thing for special bands to show themselves, for some substances may become self-luminous under special conditions at very moderate temperatures; it is quite another thing that the solid parts of a comet's substance should become incandescent. I venture to express my own belief that this can scarcely happen except in the case of comets which approach very near to the sun. Besides the continuous spectrum with dark lines, the photograph showed also a spectrum of bright lines.

> These lines (says Dr. Huggins) possessed extreme interest, for there was certainly contained within this hieroglyphic writing some new information. A discussion of the position of these new lines showed them to be undoubtedly the same lines which appear in certain compounds of carbon. Not long before Professors Liveing and Dewar had found from their laboratory experiments that these lines are only present when nitrogen is also present, and that they indicate a nitrogen compound of carbon, namely:—cyanogen. Two other bright groups were also seen in the photograph, confirming the presence of hydrogen, carbon, and nitrogen.

It is worthy of notice that, only a few days later, Dr. H. Draper succeeded in obtaining a photograph of the same comet's spectrum. It appeared to him to confirm Dr. Huggins's statements, except only that the dark Fraunhofer lines were not visible—the photograph having probably been taken under less favourable conditions.

So far, then, it seems clear that comets shine in part by reflecting sunlight, partly with light of their own; the part of the cometic substance which certainly shines with its own light is gaseous, and this gas in most comets 'contains carbon, hydrogen, and nitrogen, possibly also oxygen, in the form of hydrocarbons, cyanogen, and possibly oxygen compounds of carbon.'

But the latest comet has brought with it fresh news. Its spectrum is not like that given by the comets we have been considering. The bright lines of sodium are seen in it, and also other bright lines and groups of lines, which have not yet been shown to be identical with any belonging to the hydrocarbon groups, but probably are so. Dr. Huggins's photograph shows, he considers, 'that the original light of the comet, which gives a continuous spectrum (he means that portion of the original light which does so), was too strong to allow of the Fraunhofer lines being recognised in the reflected solar light.' We demur to this as being *shown*, it may fairly be said to be *suggested*. The cyanogen groups are not seen.

Such is Dr. Huggins's account; but it is manifest that this comet underwent important changes, of which—we are surprised to note—Dr. Huggins has taken no account. Thus, in April, Professors Tacchini and Vogel found simply a faint continuous spectrum. In May, Vogel found that the three bands associated with carbon were present, though faint, while there was no trace whatever of the sodium band. On the contrary, on the nights of June 4, 5, and 7, Dr. B. Hasselberg, of the Observatory of Pulkowa, found that the nucleus of the comet gave a very strong and extended continuous spectrum, with an 'excessively strong' bright line in the orange yellow, proved by micrometrical measurement to be identical with the D line—the well-known double sodium line of the solar spectrum. The observation was confirmed by Dunér, Bredichin, and Vogel. On this Mr. Hind remarks, 'it is necessary to conclude that, during the last fortnight of May, the spectrum of Wells's comet had changed in a manner of which the history of science furnishes no precedent.' It must, however, be remembered that as yet no comets have been examined under sufficiently favourable conditions to enable us to say whether the change thus observed was really exceptional, or only exceptional in being for the first time noted. Whenever such a comet as Donati's comes favourably under spectroscopic scrutiny, we shall probably learn something about these changes which will throw more light than anything yet discovered on the physical economy of these mysterious bodies.

What, then, do we know certainly respecting comets? What may we surmise with more or less probability? And in what direction may we look with most hope for future information? We know certainly that, in whatever way they are formed, the sun excites intense disturbance in them as they approach him. Professor Stokes remarks that these effects, so much greater at a first view than we might fairly expect in the case of many of the comets observed, which have approached the sun no nearer than our own earth does, or not so near, may be accounted for by the circumstance that comets travel in what must be regarded as, to all intents and purposes, a vacuum. From Dr. Crooke's experiments on very high vacua we may infer that there is very little loss of heat, except by radiation. Thus the heat received by the meteoric components of a comet would be much greater than might otherwise be expected. Dr. Huggins mentions, in the same connection, the remarkable persistence of the bright trains of meteors in the rare upper air, which sometimes remain visible for three-quarters of an hour before the light fades, as the heat is gradually radiated away. 'Our reasoning on these points,' he remarks, in his dry way, 'would undergo considerable modification if we accepted the views as to the condition of interplanetary space and of the sun's action which have been recently suggested by Dr. Siemens in his solar theory'—but of course we do not.

Bredichin's researches, showing that three distinct curvatures in comets' tails correspond to the winnowing out by solar repulsive action of (1) hydrogen, (2) carbon, and (3) iron, seem worthy of careful study and investigation. It accords well with spectroscopic evidence as to the condition of the matter raised in gaseous form from the nucleus; and if as yet we have had no direct spectroscopic evidence of the existence of iron in comets, we know that meteors are closely connected with comets, and that many meteors contain iron. Moreover, as unexpected spectroscopic evidence of the presence of the substance sodium, common in so many meteors, has been found in the case of one comet, we may fairly hope that under yet more favourable conditions, the presence of iron also may be recognised in the same way.

How far electricity may be looked to for an explanation of cometic phenomena, is a doubtful point among astronomers and physicists. For my own part, I must confess I share the strong objections which many physicists have expressed against the mere vague suggestion that perhaps *this* is an electrical phenomenon, perhaps *that other feature* is electrical too, perhaps *all or most* of the phenomena of comets depend on electricity. It is so easy to make such suggestions, so difficult to obtain evidence in their favour having the slightest scientific value. Still I hold the electrical idea to be well worth careful study. Whatever credit may hereafter be given to any electrical theory of comets, will be solely and entirely due to those who may help to establish it upon a basis of sound evidence—none whatever to the mere suggestion, which has been made time and again since it was first advanced by Fontenelle. Dr. Huggins says that he finds there is a rapidly growing feeling among physicists that both the inherent light (which he prefers to call the self-light) of comets and the phenomena of their tails belong to the order of electrical phenomena. An American astronomer recently wrote to him, as to American views of the self-light of comets, 'I cannot speak with authority for anyone but myself; still I think the prevailing impression amongst us is that this light is due to an electric, or, if I may coin the word' (far better not) 'an electric-oid action of some kind.' On this Dr. Huggins himself remarks:—

The spectroscopic results fail to give conclusive evidence on this point; still, perhaps, upon the whole, especially if we consider the photographs of last year, the teachings of the spectroscope are in favour of the view that the self-light of comets is due to electric discharges. Those who are disposed to believe that the truth lies in this direction, differ from each other in the precise modes in which they would apply the known laws of electric action to the phenomena of comets. Broadly, the different applications of principles of electricity which have been suggested, group themselves about the common idea, that great electrical disturbances are set up by the sun's action in connection with the vaporisation of some of the matter of the nucleus, and that the tail is probably matter carried away, possibly in connection with electric discharges, under an electrical influence of repulsion exerted by the sun. This view necessitates the supposition that the sun is strongly electrified, either negatively or positively, and further, that in the processes taking place in the comet, either of vaporisation or of some other kind, the matter thrown out by the nucleus has become strongly electrified in the same way as the sun—that is, negatively if the sun's electricity is negative, or positively if the sun's is positive. The enormous disturbances which the spectroscope shows to be always at work in the sun must be accompanied by electrical changes of equal magnitude, but we know nothing as to how far these are all, or the great majority of them, in one direction, so as to cause the sun to maintain permanently a high electrical state, whether positive or negative.

Unless some such state of things exist, Sir John Herschel's statement 'That this force' (the repulsive force forming the tail) 'cannot be of the nature of electric or magnetic forces,' must be accepted, for, as he points out, 'the centre of gravity of each particle, would not be affected; the attraction on one of its sides would precisely equal the repulsion on the other.' Repulsion of the cometary matter could only take place if this matter, after it has been driven off from the nucleus and the sun, have both high electric potentials of the same kind. Further, it is suggested that luminous jets, streams, halos, and envelopes belong to the same order of phenomena as the aurora, the electrical brush, and the stratified discharges of exhausted tubes.

All this, it will be noticed, is at present merely hypothetical. It is, however, worthy of notice that *outside* of electricity there is nothing known to physicists which seems to afford even a promise of explanation so far at least as the grander and more striking (also the most mysterious) of cometic phenomena are concerned. It may well be that with our advancing knowledge of meteors and meteor systems, the spectroscopic analysis of the next few comets of the larger and completer types—comets like Donati's comet, the great comet of 1811, and the comet of 1861—may throw unexpected light on mysteries which still remain among the most profound and unpromising problems presented to modern science.

DANGERS FROM COMETS.

THE appearance of two large comets in 1881, and the discovery of several telescopic comets announced in scientific journals (in 1882 also large comets were seen), have led many to ask whether these objects may be regarded as portents, while others, not quite so ill-informed, have yet fancied that there may be some connection between the comets and the exceptionally warm weather experienced during a portion of the summer. I propose to consider briefly here the ideas commonly entertained respecting the possible influence of comets on terrestrial weather, touching only in passing on the belief, which ought long since to have died a natural death, that comets are sent as signs of approaching misfortunes to the human race.

With regard to the last-mentioned superstition, I should in the first place notice that in former times the belief was natural enough. If we consider the way in which men in past ages regarded the heavenly bodies, we see that whether they considered comets to be members of the heavenly host or to be appearances in the upper air, they had good reasons for regarding them as portentous. Perceiving that the sun and moon, two of the seven planets of their astronomy, exercised very important influences on the earth, the moon ruling the tides and measuring the night, while the changes as well of the circling year with its seasons as of the day with its hours of morning, noon, and evening, were manifestly dependent on the sun's apparent motions, it was natural that they should regard the other planets as similarly influential, though they were not equally well able to ascertain what special effects each planet produced. Hence arose the system of astrology, a system whose importance to the men of past ages is seldom fully appreciated. In that system the fixed stars found necessarily their place, so that all the heavenly bodies ordinarily seen—sun, moon, planets, and stars—were regarded as of extreme importance to the human race, because in their ever-varying positions those bodies were supposed to exert ever-varying influences. If comets were to be looked upon (as by the Chaldeans, whose doctrine was later advocated by Seneca and others) as heavenly bodies, moving like the planets in regular paths, it was natural that to them should be assigned an influence of a special kind, corresponding to the special character of comets in all respects, in their motion, in their appearance, and in their changes of aspect. If, on the other hand, while the heavenly bodies were regarded as above or in the firmament,[1] the comets were regarded as below it, and, in fact, as suspended in and moving through our own air, it was natural that to bodies thus specially formed in a region nearer to the earth than that of the planets, either a more effective influence should be assigned because of their proximity, or else a specially portentous character. As bodies set in or placed outside the firmament, the planets and fixed stars might have other offices, men would suppose, than to influence or indicate the fates and fortunes of terrestrial races ; but bodies specially fashioned below the firmament which separated the earth from the celestial regions could have no other purpose than to warn the human race of approaching dangers, even if they did not actually themselves bring the troubles—plagues, pestilence, famine, flood, or desolating wars—by the noxious influences which they spread through the environing air.

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[1] I am satisfied that the doctrine of a firmament—a doctrine which almost all primitive or barbaric science recognises—occupies a most important position in the astrological beliefs with which we find it associated. This belief, Tylor well remarks, arises naturally in the minds of children, and, in accordance with the simplest childlike thought, the cosmologies of the North American Indians and the South Sea Islanders describe their flat earth arched over by the solid vault of heaven. Like thoughts are to be traced on through such details as the Zulu idea that the blue heaven is a rock encircling the earth, inside which are the sun, moon, and stars, and outside which dwell the people of heaven ; the modern negro's belief that there is a firmament stretched above like a cloth or web ; the Finnish poem which tells how Ilinarinen forged the firmament of finest steel and set in it the moon and stars. The New Zealander, with his notion of a solid firmament, through which the waters can be let down on earth through a crack or hole from the reservoir of rain above, could well explain the passage in Herodotus concerning that place in North Africa where, as the Libyans said, the sky is pierced, as well as the ancient Jewish conception of a firmament of heaven, 'strong as a molten mirror,' with its windows through which the rain pours down in deluge from the reservoirs above, windows which late Rabbinical literature tells us were made by taking out two stars.

It was in this way no doubt that comets were originally regarded. They were messengers of the gods to those nations who believed in many gods, angels of the Lord to monotheistic nations. It is noteworthy, by the way, that neither in Assyrian tablets nor in the Bible do we find any reference to comets as among the heavenly bodies known to men in those days. This is especially remarkable when we consider that the writers of the tablets, as of the earlier books of the Bible, manifestly believed in stellar and planetary influences. In the Fifth Tablet of the Babylonian Creation legend we read : ‘Stars, their appearance in figures of animals (constellations) he arranged. To fix the year through the observation of their constellations, twelve months (or signs) of stars in three rows he arranged, from the day when the year commences unto the close. He marked the position of the wandering stars (planets) to shine in their courses, that they may not do injury, and may not trouble anyone. . . . The God Uru (the moon) he caused to rise out, the night he overshadowed, to fix it also for the light of the night, until the shining of the day, that the month might not be broken, and in its amount be regular.

. . . . The God Shamas (the sun) in the horizon of the east. . . . . to the orbit was perfected.' No word about comets, any more than in the corresponding description in the first chapter of Genesis : 'God said, Let there be lights in the firmament of heaven, to divide the day from the night ; and let them be for signs' (their primary office in all astrological systems), 'and for seasons, and for days, and years : and let them be for lights in the firmament of the heaven, to give light upon the earth : and it was so. And God made two great lights; the greater light to rule the day, and the lesser light to rule the night; He made the stars also. And God set them in the firmament of the heaven, to give light upon the earth, and to rule over the day and over the night, and to divide the light from the darkness.' Manifestly comets were not regarded as among those bodies which God 'set in the firmament of heaven.' Yet they must repeatedly have been seen in those times, and could not have failed to attract the same sort of attention then as now. It seems possible that there may really be a reference to comets in some Bible passages which have been otherwise understood. For instance, when we remember the way in which comets have been compared, even in our own day, to swords threatening nations with punishment, it seems not unlikely that a comet may be referred to in 1 Chronicles xxi., verses 14, 15, &c. : 'So the Lord sent pestilence upon Israel ; and there fell of Israel seventy thousand men. And God sent an angel unto Jerusalem to destroy it : and as he was destroying the Lord beheld, and He repented him of the evil, and said to the angel that destroyed, It is enough, stay now thine hand. And the angel of the Lord stood by the threshing floor of Ornan the Jebusite. And David lifted up his eyes, and saw the angel of the Lord stand between the earth and the heaven, having a drawn sword in his hand stretched out over Jerusalem. Then David and the elders of Israel, who were clothed in sackcloth, fell upon their faces. . . . And the Lord commanded the angel; and he put up his sword again into the sheath thereof.' The whole account from verse 14 to the end of the chapter (the last sixteen verses) is worth studying in this connection. Compare with it the following passage from Defoe's 'Plague of London:'—'In the first place a blazing star or comet appeared for several months before the plague, as there did the year after, another, a little before the fire. The old women and the phlegmatic hypochondriacal part of the other sex, whom I could almost call old women too, remarked especially afterwards, though not till both those judgments were over, that those two comets *passed directly over the city*, and that so very near the houses, that it was plain they imported something peculiar to the city alone; and the comet before the pestilence was of a faint, dull, languid colour, and its motion very heavy, solemn, and slow; but that the comet before the fire was bright and sparkling; or, as others said, flaming, and its motion swift and furious, and that accordingly one foretold a heavy judgment, but slow and severe, terrible and frightful, as was the plague; but the other foretold a stroke sudden, swift, and fiery, as was the conflagration. Nay, so particular some people were, that as they looked upon that comet preceding the fire, they fancied that they not only saw it pass swiftly and fiercely, and could perceive the motion with their eye, but even they heard it, that it made a rushing mighty noise, fierce and terrible, though at a distance, and but just perceivable. I saw both these stars, and I must confess had had so much of the common notion of such things in my head, that I was apt to look upon them as the forerunners and warnings of God's judgments, and especially, when the plague had followed the first, I yet saw another of the like kind, I could not but say, God had not yet sufficiently scourged the city.'[1]

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[1] Defoe adds some instructive remarks indicating the tendency of men at times of great trouble to be oppressed by superstitious terrors: 'The apprehensions of the people,' he says, 'were likewise strangely increased by the error of the times, in which I think the people, from what principle I cannot imagine, were more addicted to prophecies and astrological conjurations, dreams, and old wives' tales, than ever they

We may thus find a reference to comets in other places where angels are mentioned. When the Psalmist says, 'He maketh his angels spirits, and his ministers a flaming fire,' he may perhaps have had in his thoughts those mysterious celestial visitants, which came he knew not whence, and went he knew not whither. Certain it is that a people like the Jews would not have been likely to overlook the strangest and most impressive of all the objects visible in the heavens. Nor is it at all likely that among so many historical narratives as we find in the Old Testament there would be no reference to some of those brilliant comets which were, we know, regarded by contemporary nations as strange and terrible portents. On the other hand, if the Jew regarded comets as angels and ministers of God's wrath, we can very well understand that he would speak of them always as with bated breath and by names implying their sacred and terrible office. Such at least would be the way with a Jew of religious tendencies. Others would regard comets with indifference. Indeed, Josephus remarks of his fellow-countrymen that they were not easily impressed by signs from heaven. 'When they were at any time premonished,' he says, 'from the lips of truth itself, by prodigies and other premonitory signs, of their approaching ruin, they had neither eyes nor ears nor understanding to make a right use of them, but passed them over without heeding or so much as thinking of them; as, for example, what shall we say of the comet in the form of a sword that hung over Jerusalem for a whole year?'

Of the feeling with which other nations regarded comets it is hardly necessary to speak, so strongly were they

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were before or since. Whether this unhappy temper was originally raised by the follies of some people who got money by it, that is to say, by printing predictions and prognostications, I know not; but certain it is, books frightened them terribly, such as *Lilly's Almanac*, *Gadbury's Astrological Predictions*, *Poor Robin's Almanac*, and the like; also several pretended religious books, one entitled, 'Come out of her, my People, lest ye be partaker of her Plagues;' another called 'Fair Warning,' another 'Britain's Remembrancer,' and many such.'

possessed with the belief that these objects portended trouble to mankind. But, as I have said, it was natural that they should think thus, nay, it was impossible that they could believe otherwise, so long as they held that the heavenly bodies are for signs to men. Even Seneca, who was so far in advance of the philosophers of his day as to maintain that comets like planets travel in fixed orbits, considered that comets were naturally regarded as tokens of divine wrath. 'The host of heavenly constellations,' he said, 'beneath the vault of heaven, whose beauty they adorn, attract no attention; but if any unusual appearance be noticed among them, at once all eyes are turned heavenwards. The sun is only looked on with interest when he is undergoing eclipse. Men observe the moon only under the like condition. The like is true of comets. When one of these fiery bodies of unusual form appears, everyone is eager to know what it means; men forget other objects to inquire about the new arrival; they know not whether to wonder or to tremble; for many spread fear on all sides, drawing from the phenomenon most grave prognostics.'

It would be well if our own times were free from these idle fears, for it would imply that men were freer from the debasing effects of ignorance and superstition. But I do not propose to consider here the unwisdom of the belief that bodies travelling uniformly in definite paths under the influence of the law of gravity should be regarded as special ministers warning men either of evil or of good approaching them. A man who could believe that Halley's comet, whose return was predicted within four weeks in 1759, and within a few hours (so greatly had the knowledge of the planets and of their attracting powers increased) in 1835, was a messenger specially sent from heaven on these occasions (or, by parity of reasoning, in its earlier visits to our neighbourhood), would believe anything; reasoning would be thrown away on such a one. But there is a belief, erroneous no doubt, but not altogether unreasonable, which merits such attention as is implied by refutation.

I refer to the belief that comets during their approach to the earth's neighbourhood or to the sun's may modify terrestrial weather either directly or by their action on the sun. To this belief, which, by some is regarded as worthy to be called a theory, I now propose to apply some of the tests which science employs for the purpose of ascertaining the truth or falsity of an hypothesis.

And first it is to be noticed that this theory as originally maintained was based on the old Aristotelian doctrine respecting comets, that they are generated in the upper regions of the air from a hot and dry exhalation, and so consumed. In a book which attracted great attention in the earlier part of the present century, Forster's *Illustrations of the Atmospherical Origin of Epidemic Diseases*, the author maintains that every unhealthy year since the Christian era has been marked by the appearance of a great comet, and that no great comet has ever appeared in a healthy year; manifestly believing, with the ancients, that comets act malefically by their direct influence on the air.

So soon as it was shown that the paths of comets do not carry them within millions of miles of the earth, or even of the outermost fringe of the earth's atmosphere, this faith in direct cometary action became untenable. Yet many still maintained the theory that a comet acts directly upon the earth, because they supposed that the malefic influence of comets had been thoroughly established by observation, although the manner in which this influence is exerted had been misunderstood.

There was indeed one occasion when apparently men had some reason for their fears. It is somewhat amusing, now that the fate of Biela's comet has been tolerably well ascertained, to think of the terror which that comet excited in 1832. Littrow, Professor of Astronomy at Vienna, was at the pains to publish a treatise explaining that these fears were unfounded. It had been announced that on October 29, 1832, the comet would only be about twenty thousand miles from the earth's path; and it was stated that if the earth were within twenty or thirty thousand miles of the comet's centre, 'such effects might be felt from the comet or from the enormous mass of vapour composing it (computed to be more than one hundred and fifty times greater than the mass of the earth)[1] as to destroy all animal and vegetable life.' But there would have been in reality nothing very alarming in the statement unless it had been also stated that the earth would be at the point of her orbit thus nearly approached by the comet, at the same time that the comet was passing. And as a matter of fact astronomers knew that the earth would not pass the point of nearest approach till November 30, no less than thirty-two days after the comet had gone by there. On October 29, the earth was about fifty millions of miles distant from the place where the two orbits are nearest to each other.

As to the danger of approach on other occasions Littrow wrote as follows at that time (and even his cautious utterances read strangely in the light of what is now known about the comet):—'We have already stated,' he said, 'that Biela's comet can only come near the earth when it is at its least distance from the sun, in the latter part of December. But since this proximity of the comet to the sun may just as well happen on *every other* day of the year as in December, and since its period is six years two hundred and seventy days, or about two thousand five hundred days, after a lapse of two thousand five hundred years a near *approach* (not an actual collision) to the comet is *probable.* I say merely *probable*, from which it must not be concluded that such an event actually will take place in two thousand five hundred years. This result merely means that a man might bet two thousand five hundred to ten or to one hundred that the comet will not come near the earth for the next ten or one hundred years. At the end of two thousand five hundred years there will be an equal chance that the comet will make this next approach, or that it will not. And after two thousand five hundred years the chance of its approaching the earth will go on increasing, but at so slow a rate that many thousands of years must elapse before the comet can be *really* expected.'[1]

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[1] This is quoted from a periodical of the day, viz., the *Penny Magazine* for October 20, 1832. It is hardly necessary to say that the mass of the comet did not approach this amount. Nor did the astronomers of 1832 make any such mistake as might be inferred from the passage quoted.

Since that time Biela's comet has made seven revolutions, and although it has not come near the earth (so far at least as its head is concerned), yet the comet has undergone dissolution, how produced is not known, but probably by solar action. In 1872 the earth passed through the comet's train of meteoric attendants, but some twelve weeks after the comet itself had passed the place where the earth thus traversed the family of bodies following along the comet's orbit. There was a beautiful display of falling stars, but the earth passed on wholly uninjured.

This was not, as it has been described, a passage of the earth through the tail of a comet; for the meteoric train and the tail are entirely distinct appendages, occupying very different regions in space. It is worthy of notice, however, that the earth has passed through the tail of the comet also, without serious consequences. This happened in the case of the famous comet of 1861, one of the most magnificent ever seen, though the nature of its path was such that the comet was not observed by many except astronomers. During the night, when, according to the calculations of Mr. Hind, superintendent of the *Nautical Almanac*, the earth was passing through the tail of this comet, but at a great distance from the head, it was noticed by some observers that the sky was full of what was described as a phosphorescent light. Whether this observation was trustworthy or not, it is certain that if the phenomenon had any real existence it was by no means striking. It is equally certain that no other effect was observed, and that the earth experienced no manner of mischief during its passage through that great comet's tail.

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[1] One does not quite see the force of this reasoning; or, rather, why a mathematician of Von Littrow's strength should content himself with anything so vague. What he means is probably this: Roughly the earth's period and the comet's contain respectively 365 and 2,500 days, so that a period of 365 times 2,500 days contains each period a certain number of times exactly, viz., 2,500 earth-periods and 2,500 comet-periods; hence at the end of this long period the two bodies will have returned pretty nearly to the position they had had at the beginning, and all possible variations in the manner of the two bodies' mutual approach will—speaking roughly—have been gone through.

So far as we can judge there is no danger whatever for the earth from the passage through a comet's train of meteoric attendants, or through the tail. Whether the passage of the earth directly through a comet's head would cause any mischief is as yet doubtful. From what we know of cometic structure, however, it seems unlikely that any serious harm could happen to the earth, even if she came into direct conflict with the nucleus of the largest comet. Assuming that the nucleus of a large comet consists partly of vapour, but in the main of meteoric masses such as form the train, only more closely set, there might be a downfall of large aerolites during the encounter; and if tens of thousands fell, as in the November star-shower tens of thousands of smaller bodies fall, it might well happen that here and there a life would be lost. But the earth has a large surface. She exposes a hundred million square miles to a flight of bodies reaching her in any given direction; so that even though a hundred million meteoric masses struck her, that would be but one per square mile. The chances against any meteoric mass striking a human being would be enormous, even if a meteoric shower contained many hundreds of millions of masses large enough to penetrate through the atmospheric armour of the earth.

Taking next the question whether a comet may in some other way influence the earth, as by its light, or heat, or some other emanation, science simply asks another question in reply, viz., how can such influence be produced? We can measure the light which comes from a comet, even the brightest, and we find that it is exceedingly small by comparison with the light we get from the full moon. We cannot measure a comet's heat, simply because no instrument hitherto devised is delicate enough even to afford any indication of heat from a comet. As for other forms of emanation, science knows of none which can come from a comet more than from the planets or from the moon, which are certainly not regarded as sources of deleterious emanations. In point of fact, science not only has no *à priori* reasons for supposing that a comet could produce any recognisable effects on the earth by its light, heat, or other qualities, but has every reason of that kind for believing that a comet is absolutely powerless to produce any effect, good, bad, or indifferent, on the earth or other planets.

Of course, it might well be that *à posteriori* reasons might exist for regarding comets as mischievous or dangerous. If, for instance, it had been found that the appearance of a comet was always or generally followed by certain effects, as by excessive heat, plague, or pestilence, or the like, we should hardly be able perhaps to regard the coincidence as accidental. In that case, however unlikely it might appear antecedently to the student of science that comets could mischievously affect the earth, he would be bound to inquire further, in order to see whether the connection apparently existing between comets and bad years of such and such kinds were real or not. It would require, let it be at once admitted, a great weight of evidence to force anyone really acquainted with what has been discovered respecting comets to believe that any such connection exists. This is commonly misunderstood. Many think that students of science have come to a foregone conclusion in the matter, as in the corresponding case of supposed planetary influences. In reality it is simply because the student of science recognises the enormous antecedent improbability of the popular ideas about cometary effects upon the earth, that he pays very little attention to the evidence which many persons think they find in favour of these ideas. He knows, also, better than those who have not studied the subject, what an enormous mass of facts has been gathered together, from among which, by due selection, what would seem like overwhelming evidence could be found in favour of almost any theory. It could be proved to the perfect satisfaction of all, except those who have studied the subject, that comets produce heat or cold, health or pestilence, wars and famines, or periods of peace and plenty. When we take the entire evidence, we find, as we might expect, that it is fairly balanced for all these contradictory influences, or, in other words, that there is no evidence at all in favour of cometary effects on weather, or on health, or on the relations of men and nations amongst each other.

This is, of course, no new discovery. Ever since modern science began—by which I mean science depending on systematic observation—it has been known that the idea of cometary influences has had no support in observed facts. Not to go so far back, the questions which have been asked during the past few months were asked half a century ago, and then received the same reply which science gives to them now. Thus, Von Littrow, writing in 1831 about the belief that comets make our seasons warmer, said: ‘In reply to this assertion I give the years, from 1632 to 1785, which were remarkable for the unusual temperature either of their winter or their summer, and were likewise distinguished by the appearance of comets:—

Comet years	Temperature
1632	*Hot summer*
1665	Severe winter
1680	Severe winter
1682	*Warm winter*
1683	Cold summer
1683	Severe winter
1684	Cold summer
1689	*Warm winter*
1695	Cold summer
1699	Severe winter
1701	*Hot summer*
1702	*Hot summer*
1702	*Warm winter*
1706	Severe winter
1718	*Hot summer*
1718	Severe winter
1723	*Hot summer*
1729	Severe winter
1737	*Hot summer*
1744	Severe winter
1748	*Hot summer*
1764	*Warm winter*

Comet years	Temperature
1766	Severe winter
1769	*Warm winter*
1771	Severe winter
1774	*Hot summer*
1781	*Hot summer*
1783	*Warm winter*
1784	Severe winter
1784	Severe winter

Here are thirty cases, and it happens that in exactly half (the italicised cases) the effect which would be attributed to the comet, if the comet had any effect on temperature at all, would be an increase of heat, while in the other half such effect would be a diminution of heat. It is clear, then, so far as the evidence goes, that a comet produces no effect one way or the other.'

Perhaps some reader, noticing that in twenty-two cases out of thirty the list shows either a hot summer or a severe winter, will suggest that a comet appears in general to cause either an excess of warmth in summer or of cold in winter. To this the reply simply is that cool summers and warm winters are not such noteworthy phenomena as hot summers and severe winters, and hence more of the two latter would of course be noticed and tabulated than of the two former. Indeed, if it would require a great weight of evidence to satisfy a student of cometic science that comets had any effect at all on temperature, it would require much stronger evidence (indeed, evidence quite overwhelming) to satisfy him that comets could produce opposite effects, making summer hotter and winter colder.

But though such evidence as the above was given half a century ago, and was old even then, we still find the question mooted as almost a new one, whether comets affect the weather. We had some exceptionally warm weather last July, and because a comet was visible, the blame was thrown on that celestial visitant. Another comet came, and during its visibility the weather was exceptionally cold, yet few seem to think that this evidence in one direction should be regarded as negativing the supposed evidence in the contrary direction ; while some threw out the startling (and it need hardly be said utterly unscientific) notion, that one comet caused an increase of heat, being of the warm sort, while the other, being a cold one, caused the temperature to fall. It still remains to be seen what effect the comet reported (as I write) from America will produce on the weather.

Are we then to conclude that comets bring with them no changes, to our earth or other members of the solar system? It appears to me we cannot altogether infer this, though the only form of danger which seems to exist is fortunately not very marked.

Though comets can neither injure the earth by falling on her surface or by the conflict of their trains or tails with her globe, nor by the action of their light, heat, or other such influence upon her inhabitants, they might do mischief, possibly, by their indirect action. It was long since pointed out by Newton that if a comet were to fall directly upon the sun, his heat might be so increased after the comet's downfall as to destroy every trace of life on the surface of the earth. In Newton's day the cause of the solar heat was not well understood. The sun was regarded as a gigantic fire; and the only way in which Newton, or any of his contemporaries, could imagine that a comet could increase the sun's heat was by bringing fuel to this monstrous fire. We know now that if any great quantity of combustible matter could simply be placed upon the sun's surface, his heat would be for a while diminished rather than increased, as it would be in part occupied in raising the newly arrived matter to the sun's own temperature.

But in another way than Newton had in his thoughts, a comet reaching the sun from outer space would cause an increase of solar heat; not as fuel feeding the solar fires, but as moving matter adding to the sun's activity by virtue of its motion. A comet, if of sufficient mass, might so far increase the solar heat as to do mischief to the earth and other planets, even though the actual accession of energy might be very small indeed compared with the sun's normal activity.

Rightly to apprehend the nature of this special danger, the reader should compare the statement that a comet falling on the sun might do mischief with my former statement that a comet falling on the earth would probably do no mischief at all, or very little. It might seem at a first view that the direct mischief which a comet might cause by falling directly on the earth must be far greater than the indirect mischief which it could cause the earth by falling on the sun. The reason why this is not so is that the body fallen upon has a part in the mischief-causing work—indeed, in one case produces the whole effect from which mischief may follow. A body forming part of a comet (head, tail, or train) which fell on the earth would be moving with a certain velocity when first its course brought it near enough to the earth to have its motion measurably affected by the earth's attraction. During the remainder of its course its velocity would be increased by the last-named influence, and when finally it struck the earth (supposing it able to break its way through the resistance of the atmosphere) a portion of its striking velocity would be earth-born. But in the majority of cases this portion would be small relatively as well as absolutely, and in every case it would be absolutely small. The greatest possible effect the earth could produce on a body reaching her from without would be that which she could produce if she were the only orb in the universe, and the body started from rest towards her, moving from a very great distance. Then she would give to the body a velocity of seven miles per second; that is, the body would strike her surface with that velocity. The velocity seems enormous, and is indeed some thirty times greater than the velocity of a cannon-ball. But even though thousands, or hundreds of thousands, or millions of such bodies as form the meteoric train or nucleus of a comet reached the earth with this velocity, the total effect on the earth would be insignificant (to say nothing of the protective effect of the atmosphere). As a matter of fact, the earth, not being the only orb in the universe, never can give this velocity, or a velocity nearly so great, to a body approaching her from without. Every such body is, and has been for a long time before reaching her, under the much greater attractive influence of the sun, and by far the greater part of the velocity which any such body has is sun-born. Yet even with the velocities generated by the sun *at the earth's distance*, bodies following in the train of a comet, or forming part of a comet's head or nucleus, could do little harm to the earth. It is because bodies falling on the sun are acted on by him much more effectively, that they might do harm, more harm indirectly than bodies falling on the earth itself could do directly. They cannot reach him without having been acted on by him over those parts of the planetary system which lie within the earth's orbit or nearer to him than the earth, nearer than Venus, nearer than Mercury, nearer than any planets (if such there are) which travel between him and Mercury. Not only may they be acted upon up to his very surface as we see it, but it may very well be, nay, it almost certainly is the case, that his real surface lies far below that apparent surface ; and if this is so, a body reaching his actual surface is exposed to the yet mightier influence which his giant orb must exert within that surface below which no telescope penetrates. Even at that surface a body reaching the sun from far remote space, under his own attractive influence only, would travel at the rate of 360 miles per second. The heat generated when a body moving at this rate was brought to rest would be enormous, even though the body itself were of small mass. When we remember the enormous size of the sun, that the surface turned at any instant towards a flight of bodies approaching from without is about 2,350,000,000,000 square miles, we see that if a comet's nucleus were of the larger sort, and contained many millions of millions of rocky masses much larger than those which astronomy recognises as probably forming the nucleus of Tempel's comet (the November meteor), the capture within a short time-interval of all those masses could not fail to result in a tremendous temporary accession of heat by the solar mass. For a short time—it might be for a few days only, or for a few hours even—the emission of solar heat would be greatly increased. Without any very inordinate conceptions as to the total mass of the destroyed comet, we can see that the solar heat might for a day or two be doubled or even increased in much greater degree. He would return presently to his usual condition, but in the meantime the earth's inhabitants would have suffered greatly, even if they had not been (as they well might be) destroyed altogether by excess of heat.

But, it may be said, the dangers here described are wholly imaginary. No comet of the larger sort ever has fallen, or ever can fall, on the sun. We know that thousands of comets have appeared in our skies without any such ill effects. We know also that our sun is one of many thousands of suns, all of which we must assume are equally exposed to the dangers described; yet all shine steadfastly in the heavens. Neither the comets which science has observed and studied, nor the stars whose lustre has been determined and watched, tell us anything to confirm the dismal anticipations suggested by the above considerations.

It so happens, however, that comets and stars have agreed in showing that the danger exists, though they agree in indicating that it is small and remote. Or rather, the evidence given by the stars, if it really bears on the danger we are considering, shows that the chance of mischief is small, but that should the mischief occur it would be very great, if not absolutely destructive.

First as to the evidence given by comets.

Most comets travel on paths which nowhere approach within many millions of miles of the solar orb. The effects mentioned this year as likely to have been produced by cometic action on the sun could never have been imagined by any except those utterly ignorant of matters astronomical; where persons not so ignorant suggested dangers, it was with the intention of acting upon public credulity in such matters. Every astronomer knows that not one of the comets of the year 1881 could have produced the slightest measurable effect upon the sun.

But there have been comets which have approached so near to the sun's surface as to suggest unmistakably the possibility that a comet may one day be absorbed by the sun. Such was the comet of 1668, which, according to the rough observations of Goa, in India, passed within 40,000 or 50,000 miles of the sun's surface. The comet of 1843 passed within 190,000 miles of the sun's surface according to some estimates, but according to others went nearer. When we consider that these estimates refer to the centre of the comet's head, and that a comet is not a point but a very large object, while we know that outside the visible surface of the sun the prominence region extends many thousands of miles, we see that such comets as the above-named may be regarded as having to all intents and purposes absolutely grazed the surface of the sun.

But this is far from being all. In February 1880 a comet appeared whose path was very similar to that pursued by the comet of 1843. Mr. Hind, the superintendent of the *Nautical Almanac*, examining the observations made by Dr. Gould at Cordoba, and by Mr. Ellery at Melbourne, as well as the places noted by Mr. Gill, of Cape Town Observatory, obtained in each case for the comet of February 1880 a path sensibly the same as that of the comet of 1843. Professor Weiss, of Vienna, was led to a similar conclusion; while we learn that Professor Winnecke, judging from a comparison of the orbit of the great comet of 1843 with Gould's position on February 4, and Gill's later rough ones, is of opinion that the identity of the comets of 1843 and 1880 hardly admits of a doubt.

Now the comet of 1843 was not expected to return so soon as 1880. Professor Hubbard, of Washington, assigned to it a period of revolution of 533 years. He showed, indeed, that a period of 200, or 175, or even 150 years, might be reconciled with the observations; and Dr. Gould has shown that the period of thirty-seven years, which would correspond with the return of the comet in 1880, involves no very important correction of any single observation made on the comet of 1843. Still there is this great difference between the interpretation of the comet's observed motions with the longer and the shorter periods. Where the longer periods are used the discrepancies are pretty equally distributed in different directions—one observation sets the comet slightly in advance of the position calculated from the assumed period, another sets the comet slightly behind its calculated place ; one sets it slightly on one side, another slightly on the other side of its computed orbit. But when one of the shorter periods is employed this is no longer the case. The discrepancies, though slight, are all in one direction. Every astronomer recognises the importance of this difference.

Assuming then that one of the longer periods, say a period certainly exceeding 100 years, must most probably be assigned to the comet of 1843, while yet we cannot reject the evidence showing the identity of the comets of 1843 and 1880, we are led to the conclusion that from some cause or another the period of the comet has undergone a remarkable diminution. We can hardly imagine that there are two different comets travelling in the same track. It is true we find meteoric flights travelling in the same tracks after a comet, but we have nothing which seems to render it likely, or indeed conceivable, that two comets would be associated in this way. We seem forced to accept as at any rate far more probable the conclusion that the comets of 1843 and 1880 are really one and the same object, but that the period, formerly much larger, has been reduced to thirty-seven years.

But there is only one way in which a comet's period can be reduced so greatly, viz., by a cause diminishing the comet's velocity at some point of its orbit. Moreover, the place where the velocity is thus affected must lie in or near that part of the comet's orbit which remains almost unchanged. The track pursued by the comet of 1880 during its visibility was almost precisely the same as that pursued by the comet of 1843. Hence the comet of 1843 must have been disturbed somewhere along that part of its track which thirty-seven years later was traversed by the comet of 1880. In the very midst of this part of the track lies the point where either track approaches nearest to the sun—the perihelion of the orbit as it is technically called. Somewhere near this point, most probably at this very point, the velocity of the comet of 1843 must have been reduced. Now we have seen that at this part of its path the comet was very close indeed to the sun, so close that even the centre of the head must have passed through the surface of the sun. We can understand then that the comet may here have been retarded by the resistance of the matter forming the solar appendages (the prominences and the corona), even if not still more effectively retarded by resistance experienced at the actual surface of the sun. If so retarded in 1843 the comet must have been still further retarded in 1880, and its period still further reduced. If so, it will probably return before the end of the present century, then again after a shorter interval, and so after gradually shortening intervals until before very long the comet will be finally absorbed by the sun.

Now all this implies no great danger either for the sun or the earth. If we assume that our conclusion is absolutely correct, and that the comet will before long—say in less than a century—be absorbed by the sun, still there are abundant reasons for believing that the mischief which could possibly accrue to the earth can be but small. The comet, according to our assumption, was effectively retarded in 1843. At that time no inconsiderable portion of its motion must have been transformed into solar heat. Yet we know that there was no such accession of solar heat as could be felt by all, none even that science could measure. Nor was there any such accession of solar heat in 1880, when the comet must have been still further retarded. There is then every reason to believe that whatever danger some comets may bring to the solar system, the comet of 1843 is not one of the very dangerous ones. Its course brings it menacingly near to the solar orb, but its mass and constitution appear to be such that its final absorption by the sun will not involve any serious danger to the solar system by increase of the sun's heat.

When we consider, however, how vastly the comet of 1843 has been exceeded in volume and presumably in mass by other known comets, and the wide range of disparity in splendour among comets already observed (showing that probably even the largest observed may be but small compared with some comets which exist but have not yet been seen), we see that the kind of danger shown by the motions of the comet of 1843 to be real enough, may in the case of other and much larger comets be not only real but great. Such a comet, for instance, as that of 1811, which, though it never approached the sun within 90,000,000 miles, yet displayed greater splendour and greater cometic development than comets which have all but grazed the solar surface, would be a very dangerous visitor if its course chanced to be so directed as to carry it straight towards the sun. And there may well be comets as far exceeding that of 1811 as this exceeded the comet of 1843, while the course of any comet may well chance to be so directed as to carry it straight towards the very centre of the sun instead of passing grazingly by his orb as did the comet of 1843. Of course the chance of a very large comet visiting the solar system on just such a course is exceedingly minute. Still the event is altogether possible. There can scarcely be a doubt that if the event occurred the result would be disastrous for the present inhabitants of the solar system. The downfall of millions of millions of masses, each weighing many tons (a fair supposition as to the average weight of the meteoric attendants on so large a comet as we are considering), at the rate of 350 or 360 miles per second, upon the sun's orb, could not fail to be an enormous, though short-lasting, accession of solar splendour and of solar heat, a change which could not but prove destructive to every form of life existing on the earth or any other inhabited planet.

The chance of such a catastrophe is small. It is so small that not one sun in millions might be expected to suffer in this way during thousands of years. (For we must remember that our sun is one of a very large family of suns, and that whatever danger he is exposed to, threatens presumably each member of that family.) May we not in this way test at once the reality and the extent of the danger? If any sun among the millions, the tens, nay, the hundreds of millions,[1] visible in the telescope should sustain the direct impact of a very large comet, and should thereby for a short time increase greatly in heat and lustre, that sun would during that time be visible without telescopic aid. Probably even the faintest star which the most powerful telescope can just show us, would become visible to the naked eye during such an outburst of light and heat.

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[1] It is commonly stated that within the range of the gauging telescopes of the Herschels as many as twenty million suns are visible. This estimate, due to the French astronomer Chacornac, falls far short of the truth. Argelander was able, with a telescope less than three inches in diameter, to chart more than 300,000 stars in the northern skies. From observations of my own I am satisfied that if the survey with that instrument had been carried on only upon the darkest and clearest nights at least 500,000 stars would have been seen in the northern hemisphere, or a million stars in the entire heavens, or more than 150 times as many as are visible to the naked eye. Now, at a most moderate computation, the Herschelian eighteen-inch gauging telescopes have twenty-five times the light-gathering power of the puny instrument used by Argelander. A star which would be just visible with the eighteen-inch telescope would be five times as far away as one which would be just visible with the $2\frac{3}{4}$-inch one. The stellar domain ranged over by the larger telescope would therefore be 5 times 5 times 5 times or 125 times as large as that surveyed by the smaller. Apart then from any extinction of light in its passage through space, and assuming an equal distribution of stars within the range of the larger telescope, 125 times more stars would be shown by the larger than by the smaller instrument. Now, allowing the fullest weight to the elder Struve's theory of extinction, or rather to the evidence on which it is based (which will equally well be explained by a diminishing richness of star-distribution at great distance), we yet cannot suppose that the total number of stars within range of the great gauging telescopes would be reduced from 125 to barely 20 millions. Probably there are at least a hundred millions of stars within the range of those telescopes, and a thousand millions within the range of the great telescope of Lord Rosse.

Turning to the stars to see what evidence they have given, we find that there have been occasionally just such changes among the stars as we should be led to expect from what the comets have taught us. We find that, on the one hand, some stars have suddenly increased in lustre so greatly as to pass from absolute invisibility to great brightness (in one or two cases even to a brightness exceeding that of a first magnitude star); while on the other hand these cases have been so few when the enormous number of stars is taken into account, as to show that the danger in the case of any given star is exceedingly small. Among all the hundreds of millions of suns working steadily at their task of ruling and nourishing the worlds that circle around them, not one in a million has during the last three thousand years met with an accident of the kind considered, even if we assume that every appearance of a so-called 'new star' is to be regarded as in reality a case of solar outburst, and has been in reality brought about by cometic or meteoric downfall. Considering that of two such cases submitted to spectroscopic investigation (the so-called new star seen in Cygnus in November 1876) one proved to be no new star at all, while in the other (the new star seen in Corona in May 1869), though it was undoubtedly a case in which a sun blazed for a time with several hundred times its normal splendour, the change may possibly have resulted from some other cause of danger to which our sun may not be exposed, we see that, so far as probabilities are concerned, the danger that the solar system may be ruined by a solar outburst of some sort is exceedingly small. The only kind of danger to which, so far as we can judge, our sun is exposed, that from cometic downfall upon his globe, has not yet been proved to be serious in itself; while assuming that such a cause might produce a great increase of solar light and heat for a while, we learn from the stars that the actual cases of such change among all the stars from all causes are very few in number, considering the enormous number of the stars. The chances are certainly not one in a million that our sun will undergo any change of the kind considered during the next ten thousand years, even if the sun be supposed to be antecedently as much exposed to such change as those other suns which appear to have undergone it. But the constancy of the solar light and heat during the past five thousand years, and even (judging from the geologic record) during hundreds of thousands of years, affords in reality strong evidence that he is less exposed than some at any rate among the suns to dangers of this kind. Indeed, it is worthy of notice that almost all the so-called new stars—that is, if our views are correct, almost all the suns that have undergone a change destructive to life on their dependent worlds—occupy a certain definite region of the heavens lying near the edge of the Milky Way. Taking this into account, it may be said, in fine, that the danger of our earth's destruction by fire, the elements dissolving under the fervent heat of the comet-struck sun, is so small that it may to all intents be valued at 'almost naked nothing.'

THE WORLD'S END.

Great talk among people how some of the Fanatiques do say that the end of the world is at hand, and that next Tuesday (Dec. 2, 1662) is to be the day.—*Pepys' Diary.*

In the year 1000 A.D. it was almost the universal opinion that the world approached its end. Early Mother Shiptons had indicated that as the fateful year. Satan had been chained for a thousand years, and was to be loosened when the thousand years were complete. The end of the world was to be brought about by him indirectly, for his temporary triumph was to lead to the second coming of Christ, the Day of Judgment, and the end of all things terrestrial. The anticipation of these events caused natural phenomena, such as are occurring all the time, to assume a more than usually portentous aspect. Just as last year, when, according to the Shipton prophecy, our world was to come to an end, every one who believed in the prophecy found in the weather reports from different parts of the earth proof positive, or at least confirmation strong, of the threatened end—men's hearts failing them for fear because of earthquakes, storms, and so forth, which ordinarily pass without attracting special attention; so in the year 1000 every meteorological and celestial phenomenon was anxiously watched as the possible precursor of the coming catastrophe. A comet appeared and was visible for nine days, and everyone began to ask (like Fanny Squeers), 'Is this the end?' A wonderful meteor was seen, and men's frightened fancies enabled them to see what men of science seldom have the opportunity of observing now during meteoric displays. 'The heavens opened,' we are told, 'and a kind of flaming torch fell upon the earth, leaving behind a long track of light like the path of a flash of lightning. Its brightness was so great that it frightened not only those who were in the fields, but even those who were in the houses. As this opening in the sky slowly closed, men saw with horror the figure of a dragon, whose feet were blue, and whose head seemed to grow larger and larger.' A terrible picture accompanies this description. There is the meteor track, with various coruscations and widenings, so arranged as to correspond with the figure of a dragon assigned to the portentous object; but as the resemblance might not seem absolutely convincing to unimaginative persons, a dragon to match is set beside the celestial apparition, and this creature is labelled for the benefit of the inexperienced, 'Serpens cum ceruleis pedibus.'

It is exceedingly probable that if general literature had reached as widely then as it does now, the fears entertained in the year 1000 would have surpassed in intensity those which have been engendered since that time by successive predictions of the world's approaching end. But the great bulk of the population here and elsewhere probably heard very little of these terrible forewarnings. They had many other things to attend to in those 'good old times,' and some of their surroundings might very likely have suggested that they could not be much worse off if the world should actually perish at that time. As for their betters, they also were pretty busily engaged plundering each other and fighting with such zeal that manifestly for a considerable number the end was likely to come at least as soon as the general destruction threatened by the prophets. At any rate, though we have clear evidence that many believed in the predicted end of the world (indeed it was thought very wicked to be in doubt about it), matters went on much as usual; the year 1001 began, and still the world endured, with every sign of continuing.

The belief that the world would come to an end in the year 1000 was associated with, if not absolutely derived from, a much older belief entertained by the earliest astronomers of whom any records remain to us. They considered that certain cyclic periods of the planetary motions begin and end with terrestrial calamities, these calamities being of different characters according to the zodiacal relations of the planetary conjunctions. Thus the ancient Chaldeans taught (according to Diodorus Siculus) that when all the planets are conjoined in Capricornus the earth is destroyed by flood; when they are all conjoined in Cancer the earth is destroyed by fire. But after each such end comes the beginning of a new cycle, at which time all things are created afresh. A favourite doctrine respecting these cyclic destructions was that the period intervening between each was the Annus Magnus, or great year, required for the return of the then known planets to the position (of conjunction) which they were understood to have had at the beginning of the great year. According to some this period lasted 360,000 years; others assigned to it 300,000 years, while according to Orpheus it lasted only 120,000 years. But it was in every case a multiple of a thousand years, and the subordinate catastrophes were supposed to divide the great year into sets of so many thousand years.

In Plato's 'Timæus' we have some account of the Egyptian ideas concerning these successive world-endings, though minor catastrophes only are referred to; but when Solon described to the Egyptian priests Deucalion's flood, and counted for them the generations which had elapsed since it occurred, an aged priest said to him: 'Like the rest of mankind the Greek nation has suffered from natural convulsions, which occur from time to time according to the position of the heavenly bodies, when parts of the earth are destroyed by the two great agents, fire and water. At certain periods portions of the human race perish in the waters, and rude survivors too often fail to transmit historical evidence of the event. You Greeks remember one record only. There have been many. You do not even know at present anything of that fairest and noblest race of which you are a seed or remnant.' The aged priest then read from Egyptian annals the records of events which had happened in Greece 9,000 years before; he described the founding of the city of Sais 8,000 years before; and this account, registered in their ancient and sacred records, Solon read at leisure. The most remarkable of the earth's cataclysms were there described, including the destruction by flood of the great island of Atlantis. This was described as a continent opposite the Pillars of Hercules (the Straits of Gibraltar), larger in extent than Lybia and Asia together (!), and was on the road to other islands, and to a great continent of which the whole of the Mediterranean Sea was then but the harbour. Within the Pillars the empire of Atlantis reached to Egypt and Tyrrhenia. In remote times this mighty power was arrayed against Egypt and Hellas, and all those countries which bordered on the Mediterranean. Greece bravely repelled the invaders and freed all nations within the Pillars. Some time after there was a great earthquake, and the warrior races of Hellas were drowned—the great island of Atlantis also disappeared, being submerged beneath the sea.

The conflagrations and deluges by which portions of the earth, and at times the whole earth, were destroyed, were believed to be intended for the regeneration of the world. After each catastrophe, men were created afresh free from vice and misery; but gradually they fell away from this happy state to a condition of immorality, which rendered a new decree of destruction necessary.

Lyell notes that the sect of Stoics adopted most fully the system of catastrophes thus designed for the alternate destruction and regeneration of the world. They taught that they were of two kinds—'the cataclysm, or destruction by water, which sweeps away the whole human race, and annihilates all the animal and vegetable productions of nature; and the epyrosis, or destruction by fire, which dissolves the globe itself. From the Egyptians also they derived the doctrine of the gradual debasement of man from a state of innocence. Towards the termination of each era the gods could no longer bear the wickedness of men, and a shock of the elements, or a deluge, overwhelmed them ; after which calamity Astræa again descended on the earth, to renew the golden age.'

That the partial destructions of the earth, whether by flood or fire, were associated with the movements of the heavenly bodies is evident from the fact that, wherever we meet with these ideas, whether in Egyptian, Assyrian, Indian, or Chinese records, direct reference is always made to the conjunction of the planets, the position of the sun and moon, and occasionally to the apparition of comets and the fall of meteoric bodies. The following account of the Chinese Flood, attributed to the reign of Yu, is traced in the order of Hangshan, a mountain on which for many ages annual sacrifices were made by the ancient emperors of China. 'The great and little islets and inhabited places,' says the venerable emperor of the house of Hia, 'even to their summits, the abodes of the beasts and birds and all beings, are widely inundated. I repose on the top of the mountain Yohlu. By prudence and labours I have communicated with spirits. I know not the hours, but repose myself only amid incessant labours. By the dark influence of sun and moon the mountains Hwa, Yoh, Tai, and Hang alone remain above the waters. Upon them has been the beginning and end of my enterprise. When my labours were completed I offered a thanksgiving sacrifice at the solstice. My affliction has ceased ; the confusion in nature has disappeared ; the deep currents coming from the south flow into the sea. The flood began at equinox. The skies rained meteoric showers of iron of extraordinary duration.' Some portions of the country remained under water several years until B.C. 2233, when canals ordered to be cut by the Emperor Ta Yu conveyed to the sea the immense bodies of water which had been precipitated upon and overflowed so large a part of China. By this means-river beds were finally cut, shedding water in new directions, and continued to be worn deeper by the receding flow, until the whole country was tolerably free from inundation.

Sir Charles Lyell remarks of this flood that it rather interrupted the work of agriculture than involved any widespread destruction of the human race. Mr. Davis, who accompanied two British embassies to China, points out that 'even now a great derangement of the waters of the Yellow River might cause the flood of Yaou to be repeated, and lay the most fertile and populous plains of China under water.' It is noteworthy, however, that in the ancient records the action of the sun and moon, presumably in raising tides, is mentioned, while meteoric showers are distinctly associated with the occurrence of the flood—though whether they came at the beginning of the disturbance, or simply occurred while the waters were out over the plains of China, does not clearly appear.

After the threatened but not accomplished destruction of the world in the year A.D. 1000, comets were for a while looked on with suspicion, an idea appearing to prevail that the torch which was to light the final conflagration would be a cometic one. For several centuries, however, no comet came near enough to the earth or sun to excite any serious terrors founded on observed astronomical relations. But the comet of 1680 really presented characteristics which suggested dangers even to men of science. It was a comet of remarkable appearance; its course seemed at first directed full upon the sun; and though in those days it was the erroneous idea that the comet might supply an undue amount of fuel to the central fire of the solar system, which chiefly occupied men's thoughts (even Newton sharing the idea), the danger from which the solar system then escaped was considered to be real and serious.

In the year 1773 a report got abroad—how engendered is not known—that Lalande, one of the ablest mathematicians of the day, had predicted the end of the world, as the result of a collision to take place between a comet and the earth. We say it is not known how the report got abroad. The circumstance which gave rise to the report, is, however, well known, though avowedly there was nothing in it to have suggested special anxiety. The difficulty is to connect the circumstance with the exaggerated terrors presently excited. It had been announced that Lalande would read before the Academy of Sciences a paper entitled 'Reflections on those comets which can approach the earth.' It would be difficult to inquire how the report of this came gradually to be changed into the definite news that in the year 1773—nay, the very day was named, on May 20, 1773—a comet would encounter and destroy the earth, did not recent experience show how a statement of one kind may be changed—through carelessness, not through wilful misrepresentation—into a statement of an entirely different kind, when (in its later form) it seems to indicate the approach of some great danger to the earth. Plantamour, lecturing in 1872 about comets and meteors, says that the comet of 1862 passed near the earth's orbit; that along its track are travelling millions and millions of meteoric bodies; and that when the earth crosses its track meteoric displays may be expected; adding that the next display of the kind may be expected on or about August 11 or 12. Presently the news is travelling about that on August 12, 1872, a comet will fall upon the earth and we shall all be destroyed. Who gave to Plantamour's true and innocent statement this false and mischievous form? No one can say; no one can point out where or how the true became merged into the misleading, the misleading into the incorrect, the incorrect into the utterly false. But the terrors excited were none the less real that no one could tell whence they came or how they were generated.

Once such fears have been excited, it seems useless to attempt to quiet them, at least among the hopelessly ignorant, who unfortunately are so numerous and so readily made the victims of idle terrors. Lalande published in the *Gazette de France* of May 7, 1773, the following advertisement, to quiet, as he hoped, the public mind: 'M. Lalande had not time to read his memoir upon comets which may approach the earth and cause changes in her motions; but he would observe that it is impossible to assign the epochs of such events. The next comet whose return is expected is the one which should return in eighteen years; but it is not one of those which can hurt the earth.' But this tolerably explicit statement had no effect. M. Lalande's study was crowded day after day with anxious inquirers. A number of pious people, of whom a contemporary journal made the very rude remark that 'they were as ignorant as they were imbecile,' begged the Archbishop of Paris to appoint a forty days' prayer to avert the threatened danger, which for some reason they agreed was to take the form of a mighty deluge. And he would have complied with their request only he was told by members of the Academy that he would bring ridicule upon himself and upon science if he did so.

It was at this time that Voltaire wrote his well-known 'Letter on the pretended Comet.' It ran thus:—

Grenoble, May 17, 1773.

Certain Parisians who are not philosophers, and who, if we are to believe them, will not have time to become such, have informed me that the end of the world approaches, and will occur without fail on the 20th of this present month of May. They expect that day a comet, which is to take our little globe from behind and reduce it to impalpable powder, according to a certain prediction of the Academy of Sciences which has not yet been made. Nothing is more likely than this event, for James Bernouilli, in his treatise upon the comet of 1680, predicted expressly that that famous comet would return with a terrible uproar on May 19, 1790; he assured us that its peruque indeed would signify nothing mischievous, but that its tail would be an infallible sign of the wrath of heaven. If James Bernouilli mistook, it is, after all, but a matter of fifty-four years and three days. Now, so small an error as this being regarded by all geometricians as of little moment in the immensity of ages, it is manifest that nothing can be more reasonable than to hope for the end of the world on the 20th of this present month of May, 1773, or in some other year. If the thing should not come to pass, 'omittance is no quittance' (*ce qui est différé, n'est pas perdu*). There is certainly no reason for laughing at M. Trissotin, triple idiot though he is (*tout Trissotin qu'il est*), when he says to Madame Philaminte (Molière's *Femmes Savantes*, act. iv sc. 3) :—

> 'Nous l'avons en dormant, madame, échappé belle ;
> Un monde près de nous a passé tout du long,
> Est chu tout au travers de notre tourbillon ;
> Et s'il eût en chemin rencontré notre terre,
> Elle eût été brisée en morceaux comme verre.'

'A comet coursing along its parabolic may come full tilt against our earth.' But then, what will happen? Either that comet will have a force equal to that of our earth, or greater, or less. If equal, we shall do the comet as much harm as it will do us, action and reaction being equal; if greater, the comet will bear us away with it; if less, we shall bear away the comet. This great event may occur in a thousand ways, and no one can affirm that our earth and the other planets have not experienced more than one revolution through the mischance of encountering a comet on their path. The Parisians will not desert their city on the 20th inst.; they will sing songs, and the play of 'The Comet and the World's End' will be performed at the Opéra Comique.

Singularly enough, something even more preposterous than what the great wit had thus suggested did actually occur on this occasion. The fears inspired by the predicted approach of the comet were so great that speculators took advantage of the terrors of the ignorant, and absolutely persuaded many that the priesthood had, by special intercession, obtained the privilege of dispensing a number of tickets for seats in Paradise; and these pretended tickets were sold at a very high rate. It would be interesting to inquire what idea was entertained by those who purchased these tickets as to the way in which they were to be used, to whom presented, at what time, and where.

The story to which I have just referred was quoted by a Parisian professor in 1832, when a similar scare prevailed in France. It had been announced that the comet of 1826 (Biela's) would return in 1832; and it had also been stated that the path of the comet intersected, or very nearly intersected, the path of the earth. This was immediately interpreted to signify an approaching collision between the earth and the comet, though nothing of the kind was implied. These fears, said the worthy professor, may produce effects as mischievous as those produced by the cometic panic in 1773, unless the authority of the Academy apply a prompt remedy ; and this salutary intervention is at this moment implored by many benevolent persons.

At the present time, the end of the world is threatened in more ways than one. The methods of destruction are incongruous ; but that is a detail hardly worth considering. If Scylla does not destroy us, Charybdis is bound to do the work, and *vice versâ.* There is no escape for us.

A few months ago the prophecy of Mother Shipton was chiefly feared. But as the world certainly did not come to an end in 1881 (though Gerald Massey says Mother Shipton's prophecy—which she never made by the way—was really fulfilled) we must now look for the world's destruction in other ways.

And first we see it clearly indicated in the Great Pyramid. By slightly altering the dates accepted by historians, adding a few years in one place and taking off a few years in another, it can be proved to demonstration that the number of inches in the descending or entrance passages, as far as the place where the ascending begins, is equal to the number of years from the descent of man to the Exodus ; and that the ascending passage contains as many inches as there are years from the Exodus to the beginning of the Christian era. (The rest of the descending passage, as far as the bottomless pit, or the pit with the ruin-hidden bottom—it is the same thing—clearly represents the progress of the rest of the human race downwards.) This being so, of course it follows that the grand gallery represents the Christian era. This gallery has a length of 1882 inches, or, according to recent statements (not new measurements), 1881·59. Hence, in the year 1882, or more exactly at the time 1881·59, which corresponds to 1881 years + 7 months + 2½ days, or to midnight between August 3rd [1] and 4th, the

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[1] Astronomically the second day in August ends at noon August 3.

Christian era is to end. The reader is not to be alarmed, however, by this seemingly precise statement. As the time has drawn nearer, the pyramidalists have seen fit to add fifty years (more or less, according to circumstances) during which the end is to be finally brought about; August 3 will only mark 'the beginning of the end.' Still, it may fairly be presumed that something significant will happen about that time. Possibly some remarkable person, or person who is hereafter to be remarkable, will be born at midnight August 3; in which case it seems possible that the world might remain in ignorance of the fact for a year or two.

But next the planets take their turn. The terrible words 'perihelion conjunctions' are heard with appalling effect. It is true they are entirely without meaning; science knows nothing about perihelion conjunctions; but that is nothing —any name is good enough to conjure by. Let us see what perihelion mischief is in store for us.

Jupiter was in perihelion on September 25, 1880! 'The perihelia of other planets in 1881 occurred' (this is not a scientific mode of presenting the matter; but that is not the fault of the prophets—they speak as correctly as they can) 'as follows: Mercury, February 21; Venus, March 6; Mercury, May 20; Mars, May 26; Mercury, August 16; Venus, October 16; Mercury, November 12.' This was very dreadful; though somehow the earth escaped that time. Imagine Mercury being four times in perihelion in one year. We may perhaps find an explanation in the circumstance that he completes the circuit of his orbit more than four times a year, and must pass his perihelion each time; but science tries to explain everything, and we must not be too precise in such matters. The year 1882, in which we are more interested, is even worse. Mercury has already been in perihelion, viz. on February 8; then we have March 25 (April 9?), Uranus; May 7, Mercury; August 3, Mercury; October 29, Mercury again; and absolutely on December 6 Venus transits the sun's disc! Something will surely come of this, if we only live to see it.

But worse remains behind. 'In August 1885, Saturn will be in perihelion!' 'Neptune is in apparent perihelion' (whatever that may mean) 'from 1876 to 1886, the height (?) being about 1881½!' 'Those skilled in astronomy inform us it is fully 6,000 years since the occurrence of a similarly powerful situation, although conjunctions and perihelia have occurred at more frequent intervals of time. To form an approximate opinion of what the earth is liable to experience at such periods, we must review the records of effects attending similar situations, remembering that with the ripening of our planet the effects upon the earth and its inhabitants will be more generally distributed.'

This being so, these perihelia occurring in so unusual a way, being also rendered very terrible by being called perihelion conjunctions, and the dependence of terrestrial disturbances on planetary motions being too obvious to be worth proving, we have only to consider what has happened during past floods, earthquakes, and so forth, to see exactly what is in store for us pretty soon. Science, which is always too particular in such matters, may perhaps show that whatever influences the outer and larger planets may produce on the earth (it is very doubtful whether they produce any except very slight deviations from her mean track) cannot be effectively greater when the planets are in perihelion than when they are in aphelion; that terrestrial disturbances have nothing whatever to do with these relations; and that as perihelion passages and planetary conjunctions are occurring every year, earthquakes and floods could not possibly occur in years when there were no such phenomena: but the prophets have nothing to say to all that; they calmly go on to describe the various terrestrial disturbances which have occurred, regarding any attempt to show that there is the slightest real connection between the planetary movements and the earth's throes as quite unnecessary.

Here, however, is the summing up of the planetary prophecies by one of the most earnest, and therefore wildest of the prophets. 'In cases of planetary attraction, the earth's crust becomes attracted as a solid whole. Its fluid and aerial envelope responds when irregularly attracted, by oscillating in high and low tides, alternating with unequal pressure. We are approaching both stellar and planetary conditions which fortunately will require a certain number of years—say 1880 to 1885—for their complete unfoldment; hence their action may not be wholly manifest in a special month of any year; but this whole cycle of years is liable to be affected by a generally disturbed condition of the earth and its inhabitants.'

But utter rubbish as all this is—the offspring of sheer ignorance and hysteric vapours—it is not much more absurd than the prediction recently based on the observed fact that the comet of 1880 travelled along the same path as that of 1843, this path lying very close indeed to the sun. Assuming, as is really not improbable, that the comet of 1843 passed so near to the sun as to have been retarded by the resistance of the corona, and so came back after a shorter circuit than it had before traversed, it is likely enough that the comet will next return after a yet shorter interval. Possibly Marth's period—'say seventeen years,' he puts it—may be nearer the truth, in which case the comet would come back in 1897. The next return after that might be in seven or eight years, say in 1904. The next perhaps is three or four, and very likely by about the year 1920 or 1925 that comet may reach the end of its career, being finally absorbed by the sun. It is also very likely that if, instead of being thus gradually checked off, so to speak, this comet in its original full-sized condition, with many millions of millions of meteoric attendants, had rushed full tilt upon the sun, it might have done a deal of mischief. A very able astronomer, Professor Kirkwood, of Bloomington, Indiana, believes (and very likely he is right) that two of the larger meteoric attendants on this comet falling into the sun in September 1859, produced that remarkable solar disturbance which was accompanied by very remarkable magnetic disturbances and auroral displays all over the earth : so that doubtless the whole comet with its attendants pouring all at once upon the sun would have stirred him in a way which we should have found very noteworthy, even if we did not find it absolutely destructive to the earth and its inhabitants. But as a mere matter of fact (and so counting for something, whatever end-of-the-world prophets may imagine) the comet of 1843 and 1880 does not travel full tilt upon the sun, and can never do so ; its meteoric attendants are not all gathered in a single cluster, but form an immensely long train (if Kirkwood was right in the above quoted surmise, those which fell into the sun in 1859 were at least sixteen years behind the main body) ; and it is clear that a very effective interruption of the comet's career in 1843, repeated in 1880, can take place without in any appreciable degree affecting our comfort, still less our existence. If the comet of 1880 was the same object as the object of 1843, it showed very evident signs of having suffered grievously during its former perihelion passage. If it is proportionately reduced at its next return, we might even see it fall straight upon the sun (were that possible) without much fearing any evil consequences. Nothing which is known about comets in general, or about this comet in particular, suggests the slightest danger to the solar system, though everything suggests that the comet's career as an independent body will before very long come to an end. If the comet ever was a dangerous one, owing to the concentration of its meteoric components, it is not so now. If it really has been effectively checked in its career, it is evident such interruption can take place without harming us, and therefore the final throes of the comet need not trouble us in the least. If it has not been effectively interrupted, then the end is not nearer—in any appreciable degree—now than it was in 1843 or in 1668. In any case, the end of this comet's career, whether far off or near at hand, will in all probability take place in such a way that terrestrial astronomers will never know of the event.

THE MENACING COMET.

In 1881 a dismal report appeared, to the effect that the comet of 1843, which was supposed to have returned in 1880, would come back again in 1895 and bring about the end of the world. The origin of the report was not altogether clear. At least it was not altogether clear to the writer of these lines, who, if the report had had any legitimate foundation, should have known something about it. It seems that a remark to the effect that the comet of 1880 travelled in the same orbit as the comet of 1843, and was probably the same body, but that if that were the case, it had returned long before it should have done, so that the period of revolution seemed to be shortening, had been to some degree misapprehended.

It had been suggested by several Fellows of the Royal Astronomical Society that if the comet of 1880 were really the same as that of 1843, the next return might occur in a very few years; perhaps, said Mr. Marth, in about fifteen; and each return thereafter at shorter and ever shorter intervals. For the path of the comet carries it in very close proximity to the orb of the sun; and it is generally believed that a retardation of the comet's motion must occur at each return to the sun's neighbourhood, for the simple reason that the comet can hardly be supposed to get through the matter which forms the sun's corona, without encountering some resistance. The more the comet is retarded by such resistance, the faster it will travel round its orbit—paradoxical though this may sound. At each return it will encounter more and more effective resistance, until at length it must be absorbed into the body of the sun.

Whether such absorption would produce any great effect or not upon the sun, and through him upon the solar system, was a question which to many seemed answerable only in one way. Newton had pointed out that comets might serve as fuel to the sun, and perhaps produce disastrous effects in that way, by unduly increasing the solar light and heat. 'A comet,' he said, 'after certain revolutions, by coming nearer and nearer to the sun, would have all its volatile parts condensed, and become a matter fit to recruit and replenish the sun (which must waste by the constant light and heat it emits) as a faggot would this fire if put into it.' (He was speaking to Mr. Conduitt at the time, beside a wood fire.) 'And that would probably be the effect of the comet of 1680 sooner or later; for by the observations made upon it, it seemed to have a tail of thirty or forty degrees, when it went from the sun. It might, perhaps, make five or six revolutions more first; but whenever it did, it would so much increase the heat of the sun, that this earth would be burnt, and no animals in it could live.' 'He took the three phenomena seen by Hipparchus, Tycho Brahé, and Kepler's disciples,' he added, 'to have been of this kind; for he could not otherwise account for an extraordinary light, as those were, appearing all at once amongst the fixed stars (all which he took to be suns enlightening other planets, as our sun does ours) as big as Mercury or Venus seems to us, and gradually diminishing for sixteen months and then sinking into nothing.'

But although what we now know respecting the mass of comets is by no means so much opposed to these views as many seem to imagine, our knowledge of the way in which the sun's heat is maintained will not permit us to adopt Newton's opinion. Nor will the accepted views as to the origin of the sun's heat justify us in accepting a belief in more than a very moderate accession of heat as likely to accrue, under any influences due to comets now actually travelling around the sun. All those which have passed once round the sun's immediate neighbourhood, can pass again, and yet again, with effects which can never greatly exceed those produced at their first passage. If at any one perihelion passage a comet is slightly retarded, it will be slightly retarded again at its next passage close by the sun, somewhat more at the next return, and so on continually, until it is finally absorbed, the interval between these passages continually diminishing. Only in the case of great retardation at one passage, will the retardation at the next perihelion passage be markedly greater; but in this case the effects at the earlier passage should have been noteworthy; so that as no noteworthy sudden accession of solar light and heat has ever been observed, no such earlier passage has yet occurred which should make us seriously fear the next passage of the same comet by the sun's neighbourhood.

The fears entertained, therefore, respecting the next return of the comet of 1843 are without foundation. If that comet was really so checked in speed in 1843 that it returned in thirty-seven years instead of the much longer period assigned to it by the best astronomers, then we had an opportunity at that time of estimating the effect of such interruption of the comet's motion. But no effects were then perceived. The sun was neither brighter nor hotter than usual. The inference is, then, that that frictional resistance cannot appreciably affect the sun's condition. In 1880 we had a repetition of this experience—assuming that the comet of 1880 was the same body. The sun in 1880 shone much as he had done in 1879, much as he did later in 1881 and 1882. So that the world might await with calmness the future returns of this sunlashing comet, satisfied that whatever effect might be produced on the comet, very little would be produced on the sun or the solar system.

But now suddenly news comes that a comet has been seen which American men of science have identified with the comet of 1843 and 1880, so that from thirty-seven years the period has dwindled to little more than two years and a half (more exactly 2 years, 7 months, and 21 days), which would leave us every reason for believing that the next return would occur in a few months, and the final absorption of the comet by the sun a few weeks later. And an English astronomer of deserved repute has done something more than endorse these ill-omened predictions; he has pretty clearly indicated his opinion that the approaching destruction of the comet portends events of the most serious import to this earth and all who dwell on it; that, in fact, the time is drawing near when Prospero's prediction is to be fulfilled that—

> The great globe itself,
> Yea, all which it inherit, shall dissolve,
> And like an insubstantial pageant faded,
> Leave not a rack behind.

'Could there have been anything more heartbreaking to all astronomical souls,' writes Professor Piazzi Smyth, Astronomer Royal for Scotland, 'than the uninterrupted cloud by day and by night of our unfortunate climate, ever since the announcement of the brilliant daylight comet of Monday, September 18? Tuesday, Wednesday, Thursday, Friday, Saturday, and their several nights, have each and all been uniformly utterly covered in with thick impenetrable clouds. And yet we ought to confess that one other thing might have occurred even so as to make that cloudy appearance more aggravating, more grievously disappointing still. That one overtopping culmination of misfortune would have been'—if the comet had been announced as approaching instead of receding.

It will be seen that the Astronomer Royal for Scotland regards the comet in question as a rather important body. It is not an everyday comet whose approach is so important that failing to see it must be regarded as an 'overtopping culmination of misfortune.' Now *this* comet seems to be none other than *that* comet. The body, or collection of bodies (for so rather must a comet now be regarded), which was visible to the naked eye on September 18 close to the sun—'a yard or so from the sun,' writes one startled observer—is no other than the comet of 1843, whose tail stretched half across the heavens, and which—like the comet of last month—was seen in full daylight; nay, even 'close by the sun.'

Rightly to apprehend the significance of this portent, as viewed by Professor Smyth and many others, chiefly—unlike him—unscientific persons, we should inform our readers that in this year, according to the prophecies symbolically indicated in the Great Pyramid, the end of the dispensation which began 1882 years ago is in some way as yet unknown to be brought about. Some celestial body, 'the star in the East' of the Magi, appeared then: for aught we know it may have been the same comet, and the Wise Men of the East saw in it evidence that a new dispensation was about to begin. It was fitting, then, that this year, which has now been for several years announced as the time of the end of that dispensation, a similar celestial appearance, or the same body, perhaps, should announce 'the beginning of the end.' We cannot reasonably doubt this, for careful measurement shows that the Grand Gallery in the Great Pyramid is 1,882 inches long; these inches being each the twenty-fifth part of the sacred cubit, which Pyramidalists assure us is the limit of length in that marvellous structure. Moreover, it was not altogether an accident or a mere coincidence which brought the British army to the feet of the Great Pyramid at the very time—perhaps at the very hour—when the great comet was passing its perihelion. On September 13 the British cavalry entered Cairo; on September 18 the great comet could be seen with the naked eye (though it had passed the time of its greatest splendour, described by Professor Smyth as the 'ecstatic display at perihelion passage'), and was then beginning to recede. What more natural than to suppose that as the vanguard of Sir Garnet Wolseley's army approached the base of the Pyramids, the great comet was in the very ecstasy of perihelion glory, rushing through the richest portion of the sun's coronal streamers, molten by the solar heat, resisted by the densely aggregated meteor-streams, but so retarded that its return will be hastened, and that in a few months it will come back to effect the final purpose of its existence ! If any doubt could be entertained on the subject, it should be removed by the consideration that the British nation has been proved, to the satisfaction of nearly all true believers in the Great Pyramid prophecies, to be no other than the lost ten tribes of Israel.

If this sounds a little strange—or, shall we say, the least little bit premature—let the following words by the Astronomer Royal for Scotland, by no means the least able of our astronomers, and *facile princeps* among Pyramidalists, be carefully considered.

'What comet,' he asks, 'was this? The little that was seen on Monday, September 18, is not enough to give any clue, and no London journals, *whether scientific or political*, which I have seen up to September 23, throw any light on the matter. But a note by cable from America, if fully correct, is of *profound import*. Indeed, *nothing so important to all mankind has occurred before, through eighteen hundred years at least of astronomical history*. And there is this prospect of the statement being true, that it is given under the name of Professor Lewis Boss, one of the most able and learned mathematical astronomers of the Union, and, we may say now (such has been the rapid progress of astronomy during the last few years in that country), of the world. He is said, then, to have concluded from his observations that the comet of last Monday was the comet of 1880 and 1843. A comet on each of these occasions was recognised to have passed closer to the sun's surface than any other known comet. But why has it come back so soon? In 1843 it appeared to be moving in an orbit of 170 years, and yet it came back in 1880, or in only 37 years. That was startling enough, though only looked on by the world as a case of failure of astronomical prediction. But having gone off in 1880 on an understanding generally come to by the best astronomers in Europe, North America, Rio Janeiro, the

Argentine Republic, and Australia—at all which latter places it had been well observed—that it was not to return before 37 years (and other comets, such as Halley's, and Encke's, keep to their times of revolution round the sun nearly uniformly for centuries), behold this comet has returned now, on the strength of this cablegram from America, in two years. In which case, who can say whether it may not be back again from space in a few months ; and then, not merely to graze close past, but actually to fall into the sun, which is so evidently increasing its hold upon it at every revolution? Wherefore we may be near upon the time for witnessing what effects will be produced when such an event takes place in the solar system, as astronomers have hitherto only distantly speculated upon, and no mortal eye is known to have ever beheld.'

This brings the matter home to all of us, indeed. Astronomers like Newton have distantly speculated upon the effects which would be produced if a comet fell into the sun. I fear that I have not altogether refrained from such speculations myself. Indeed, the misapprehension to which I referred at the beginning of this paper arose chiefly from such speculations of my own. For speaking, not of such grazing contact as may occur in the case of the comet of 1843 and 1880, but of such direct impact as *may* through some unlucky chance occur in the case of some comet which comes to our sun from interstellar space, I have expressed the opinion that such impact may raise the sun's heat temporarily to such intensity that every living thing on this earth would be destroyed, though the increase of heat might not last more than a few weeks or even days. I also expressed my belief (entertained before I had heard that Sir Isaac Newton, in conversation with Mr. Conduitt, had expressed similar views) that the appearance of so-called 'new stars' can only be explained by the downfall of meteoric and cometic matter upon some sun like our own, which up to that time had been steadily pouring forth heat and light to nourish the worlds circling around it. This opinion, chancing to be expressed in the closing paragraph of the same paper in which I had indicated my belief that the comet of 1880 really was the same body as the comet of 1843, returned before its time, and that this body would return next after a yet shorter interval, led many to imagine that I had expressed the opinion that the comet of 1843 and 1880, returning soon, would cause our sun to blaze out with greatly increased splendour, and so to destroy all living creatures on this earth.

Now the actual risk from the destruction of this comet by the sun I believe to be very small indeed. But as to the identity of the comet which passed its perihelion on September 17 last and the comets of 1880 and 1843 there is, I think, little room for doubt. I have carefully compared the observed positions on September 17, 18, 19, 22, 24, and 29, with the known orbit of the comet of 1843, and they all agree so closely as to leave no doubt that the new comet is travelling in the same track, so far as the part near the sun is concerned. But I note one circumstance which seems hitherto to have escaped attention. Although the course of the new comet as it passed away was on the right track, the comet was not making nearly so much way as it should have done, if moving in the reduced period of $2\frac{2}{3}$ years, or even in one year, or in half a year. In other words, the reduction of speed experienced by the comet last September was such that the comet will be back within four or five *months*, possibly in less time still than that. It may be that the observations (up to the day of my writing this, which of course precedes by several weeks the day when these words can be read) have been insufficiently exact for accuracy in this respect. But if they can be trusted, the comet will be back in a very short time indeed, possibly before the end of the year. [It was soon found that the comet will not return for many years.]

Be this as it may, it is certain that the splendid comet seen on September 18 and 19 close to the noonday sun, although not seen under conditions at all favourable to ordinary observation, gave of all the comets seen in this century, nay, of all ever seen by man, the fullest promise that one day cometic mysteries will be interpreted. An observation was made upon this comet successfully, which, repeated on similar comets more favourably situated, will give information such as astronomers have long regarded as essential to the solution of cometic mysteries, but such also as they have hitherto scarce dared to hope for.

It is of course known to all who have followed the progress of recent scientific research, that nearly all the comets which have been observed during the last score or so of years, have given under spectroscopic analysis such evidence as shows that a portion of their light comes from glowing gas. Two distinct cometic spectra have been observed—Dr. Huggins, *facile princeps* among British spectroscopists, first noted them in the case of Brorsen's comet, and of Winnecke's—each consisting of bright bands. In one case the bands have not been identified with those belonging to any known terrestrial substance; but the other and more common cometic spectrum agrees with one which has been found to be characteristic of certain compounds of carbon. 'The general close agreement in all cases,' writes Dr. Huggins, 'notwithstanding some small divergencies, of the bright bands in the cometary light with those seen in the spectra of hydrocarbons, justifies us fully in ascribing the original light of these comets to matter which contains carbon in combination with hydrogen.

These spectra of bands had been seen so systematically from 1864, when Donati made the first rough observations of the cometic spectrum, until Wells's comet was observed a few months ago, that astronomers began to think that they would get no other information from comets. It was especially unsatisfactory that no bright or dark *lines* could be seen. For in the case of one particular class of spectroscopic observations, which seemed specially likely to give interesting information about comets, bright *bands* in the spectrum are absolutely useless. We refer to those observations which indicate rapid motions of recession or approach, by displacements of the spectrum. Such displacement is always exceedingly small even in the case of bodies moving at the rate of twenty or thirty miles per second. It therefore cannot possibly be determined by observing a spectrum of broad bands of light, with no well-defined edges by which to recognise displacement.

But Wells's comet in the spring of 1881, though it attracted no special attention from ordinary star-gazers, showed for the first time a new and promising feature. This comet, which had shown the carbon bands like other comets during the first month or two of its approach towards the sun after its discovery, began, when it drew within a certain distance from him, to show evidence of the presence of glowing sodium. A few days later the pair of orange lines in the spectrum which indicate the presence of this widely distributed element, were very bright and distinct, and they continued so until the comet passed out of view from our northern heavens.

Now there was double promise in this observation. First, it showed that the changes of appearance which a comet undergoes as it draws nearer to the sun are accompanied by changes of physical condition with which the spectroscope can deal. Secondly, as the bright lines of sodium are well-defined, and as their proper place in the spectrum is known, there was promise that hereafter observations might be made to determine movements of recession or of approach which may be taking place either in different parts of the comet, or in the comet as a whole.

Let us first consider the application of spectrum analysis to determine the changes taking place in the physical condition of a comet.

It is obviously a most promising circumstance that evidence should now be attainable to show what is the real physical constitution of those different parts of a comet which present such striking changes as the comet approaches the sun. Hitherto all that has been seen has been the raising up of luminous envelopes on the side towards the sun, and the apparent sweeping away of the matter thus formed into the strange appendage called the tail. But hereafter, in the case of any comet which like Donati's (in 1858) exhibits under favourable conditions the various changes due to the increased proximity of a comet to the sun, it will be found possible to recognise by means of the spectroscope the substances which are successively volatilised as the comet moves towards the perihelion. It may possibly be found that when a comet shows, as Donati's did, several envelopes one within the other, the luminous vapours forming these are of different substances. The constitution of the tail, too, may be found to vary as the comet changes in position. Where there are more tails than one, as in the case of Donati's comet, and of other celebrated comets, the spectroscope may indicate varieties of physical structure and condition. Possibly, Bredichin's theory, that three different substances—iron, carbon, and hydrogen—are driven from the sun with different velocities, for the several tails of such comets, may be established by the spectroscopic analysis of these appendages. It may very probably be found, also, that even in the case of a comet with but a single tail, the physical constitution of the tail varies in different parts of its length.

But the possibility that movements in the nucleus, coma, and tail of a comet may be detected by spectroscopic analysis, is yet fuller of promise.

Let us briefly consider the nature of this method of observation.

When we approach a point from which waves of any sort are moving, we cross the waves in more rapid succession, and the *effect* is as though they were narrowed. When, on the other hand, we recede from their source, so that the waves (moving, it is understood, more quickly than we do), overtake us, they pass us in less rapid succession, and the effect is as though they were made broader. (We speak, of course, of their width as measured from crest to crest.) We can easily see that this would be so in a sea across which waves were swiftly travelling, a stout swimmer urging his way so as either to meet them or to be overtaken by them. It has been shown, also, experimentally that this is true of sound. When we approach a source of sound, the tone is raised (or rather appears to the ear to be so, for, of course, the sound-waves on which the tone depends are not really altered), whereas when we recede from the source of sound the tone seems lowered. This observation, indeed, may readily be made by any observant person in railway travelling; for it will be noticed that whenever the whistle of a passing engine is sounded the tone falls suddenly, or seems to do so, at the moment when the engine which had been approaching begins (having passed him) to recede. In the case of light, it was long since pointed out by Doppler that a similar effect should be produced, if only the velocity of approach or recession is not too small to be appreciable when compared with the tremendous velocity of light—186,000 miles per second. The effect would theoretically be a change of colours in the case of light really of a single pure colour. For light belonging to the red end of the spectrum is formed by waves of greater length than those which form light belonging to the violet end of the spectrum; and the various colours of the spectrum from the red to the violet end have wave-lengths gradually diminishing from the greatest length at the red end to the least length at the violet. Doppler was bold enough to hope that by this method the colours of the stars might indicate stellar movements of recession or of approach. But of that he should have seen, had he reasoned the matter aright, there was no hope or even possibility. For the light of a star contains rays of all colours from red to violet, and rays beyond the red on one side, and beyond the violet on the other, which therefore no eye can see. The only effect of any diminution of all the wave-lengths would be that a part of the violet light would be lost as light, but its place would be taken by light from the indigo, that by light from the blue, and so on, the light from the red which became orange being replaced by rays otherwise invisible from beyond the red. And similarly (only the change would be in the other direction) in the case of an increase in all the wave-lengths.

But it was early shown (so far as I know, I was the first to refer to the matter publicly, but Dr. Huggins—unknown to me—was working at the very time on the plan indicated) that the lines in the spectrum would be shifted—towards the red in the case of recession from the source of light, and towards the violet in the case of approach towards that source. This displacement can be measured—if great enough, or rather, if not too small ; for, in the case of all such motions as are taking place among the stars and planets, the displacement must be very, very small indeed.

Now to comets more than to any other class of celestial bodies this method might, it would seem, be advantageously applied. For not only do comets themselves move during a part of their course around the sun with enormous velocity, but within the comet itself changes take place which seem to imply enormously rapid motions. In particular the development of the tail, although it has not been absolutely demonstrated to be due to repulsive action, yet seems explicable in no other way ; and if it is thus caused, the movement of the matter forming the tail must take place with a velocity bringing it well within the application of the spectroscopic method.

But it is essential for the use of this method that the spectrum of the moving body should have well-defined and recognisable lines. Bands, such as those in the spectrum of the comets first observed, are utterly useless for this purpose. Their precise position cannot be determined so that we could be sure of any displacement due to motion. For this purpose we must have a line, which, when the spectrum is brought side by side with that of a terrestrial substance showing the same line, will be in line with this if the celestial source of light is at rest, and will be recognisably displaced towards the red or towards the blue if that luminous body is receding or approaching respectively.

So that when, last May, Wells's comet suddenly began to show the well-known lines of sodium, promise was at once, and for the first time, afforded, that the problems of cometic changes, in so far as these depend on motions taking place within the comet itself, may before long be solved. We can have very little doubt, for instance, that if such a comet as Donati's were now to appear, and to be studied under favourable conditions during those parts of its course in which it was subject to the most intense disturbing action, the bright lines which would be seen in the comet's spectrum would either by their displacement tell us that the substance of the comet is driven wildly hither and thither in the head and swept swiftly away to form the tail, as it *seems* to be, or else, by remaining unchanged in position, would show that there are no such movements of disturbance or repulsion.

Now the comet which has recently been seen near the sun has been observed by this method. On September 18, when it was but three degrees (say half-a-dozen sun-breadths) from the sun on the sky, it was examined in the clear sky of Nice by M. Thollon, a skilful French spectroscopist.

The spectrum, notwithstanding the obviously unfavourable conditions under which the observation was made, showed clearly the line (or rather the double line) of sodium. Here, by the way, was at once evidence such as in former times no astronomer could have of the comet's real position in space. Formerly if a comet was observed anywhere, once only, nothing could be certainly known respecting its position, except that it was somewhere in the line of sight in which it was seen. But if we are right in believing that the sodium in a comet is only vaporised and rendered self-luminous when the comet is near the sun, then the new comet on September 18 was not only shown to lie in a certain direction, but within certain tolerably narrow limits of distance.

But Thollon observed something else, not quite so satisfactorily as to be absolutely certain of it, but still so as to give a considerable degree of assurance. He says that the line of sodium seemed displaced towards the red. This would indicate recession. Observe here again how the spectroscopic method of determining motions of recession or approach may come in to help the astronomer to determine the position of a comet. Supposing this method should ever be so improved that the exact rate of a comet's motion might be determined by it, then instead of merely ascertaining, in any single observation, the direction in which a comet lies at the moment, the astronomer may learn its direction, something (as we have seen) of its distance, and the rate at which it is moving from or towards the observer. The rate of its thwart motion cannot of course be inferred from the spectroscopic observations directly, yet indirectly it can. For the rate of motion at any given distance from the sun for an orbit of known dimensions is known; now the distance of the comet being partly indicated by the spectroscopic observations, the thwart motion is known within the same degree of error. Hence, combining this with our more precise knowledge of the motion of recession or approach, we make a first rather rough approximation to the real motion, both in direction and in amount—which would determine the orbit absolutely. Observations made a day or two later will show whether the body really is moving in this orbit; and if the later observations include spectroscopic ones we shall obtain means of testing and correcting the first estimate of the orbit, which will practically give us the orbit correctly—much more correctly, at any rate, than it can be deduced by the methods at present in use from observations made on four or five different occasions.[1]

It may be well, perhaps, in conclusion, to inquire how the comet would actually be absorbed by the sun.

First, then, be it noticed that *at first* there would be no tendency towards a diminution of the perihelion distance of the comet, as many seem to imagine. The point of nearest approach must remain nearly at the same distance from the sun at each return of the comet, so long as the orbit remains eccentric. Only when the velocity in perihelion (or at the point of nearest approach) is so reduced that the centrifugal tendency no longer balances the centripetal force, would there be any approach towards the sun. This amounts to saying that until the orbit is transformed into a circle (when there will be no perihelion at all) there will be no approach towards the sun. When that transformation is effected, there will be approach at every part of the circuit—in other words, the course of the comet will become a spiral, the coils of which will draw closer and closer in towards the sun's surface : the sun will be within the coils, but the comet itself will be in the toils, and its end not far off. As throughout this approach the comet's substance would be in the form of vapour, there would probably be a rapidly increasing resistance, and hence a rapidly increasing rate of approach towards the sun. Oddly enough, the comet's rate of travelling would be increased notwithstanding this constant resistance, the sun's indrawing action adding more motion than the frictional resistance subtracts. For several days, probably, the comet in each circuit, when off the solar disc, would be a conspicuous object to spectroscopists, though not perhaps visible through the telescope. The comet would appear outside of the sun's disc, first on one side, then on the other, at intervals of about $1\frac{3}{4}$ hours—$3\frac{1}{2}$ hours being the time of circuit of a body close to the sun's surface. As this surface is carried round once in about twenty-five hours, there would be considerable loss of velocity, and resulting heat, in the substance of each part of the comet as it was absorbed. But I believe the whole heat of the sun would be little increased if the whole of the comet were thus absorbed at once ; and very little indeed if, as is certain, the absorption took place piecemeal.

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[1] Theoretically the orbit of a comet can be deduced from three observations; but practically many observations are required to give anything like accuracy.

JUPITER'S SATELLITES.

Jupiter surpasses our earth more than 1,400 times in volume. Saturn alone can be compared with him in this respect, but even Saturn is but half as large as Jupiter. In mass, this superb planet is not merely 'facile princeps,' but exceeds much more than twofold all the other planets taken together. We may view, indeed, in Jupiter and his system, a miniature, but instead of being a miniature of our earth it is as a miniature of the whole solar system that he is to be regarded. The sun himself does not so greatly exceed Jupiter in volume as Jupiter does our earth. And the bodies which circle round Jupiter travel with velocities comparable with those of the swiftest members of the solar system. While Mercury and Venus travel 100,000 and 80,000 miles an hour, and our earth travels 68,000 miles an hour round the sun, Jupiter's inner satellite travels upwards of 40,000 miles an hour around its primary. Mars travels 55,000 miles an hour round the sun ; the second satellite travels 32,000 miles an hour round Jupiter. Jupiter himself sweeps less swiftly round the sun than these satellites do around him, so that through a portion of their orbits they are actually retrograding. The third satellite also travels so swiftly round Jupiter as to be reduced very nearly to absolute rest when its velocity acts in a contrary direction to that of Jupiter. The fourth satellite travels less swiftly than the third, but yet as swiftly as the planet Saturn in his orbit around the sun.

Nearly every celestial object has an interest attaching to it other than that derived from its physical aspect—an interest which may be called historical. In the moon, for instance, we see an object without which (it is not too much to say) astronomy would never have approached its present state of exactness and accuracy. Mars, in like manner, afforded evidence such as no other planet could supply, when Kepler was engaged in the series of researches which rendered his name illustrious, and without which Newton's views might never have been directed to gravitation as a universal principle. Venus is connected with the determination of the fundamental element of all astronomical measures—the sun's distance from the earth. Mercury, Saturn, Uranus, and Neptune, the sun, fixed stars, comets, asteroids, and nebulæ, all have their historical interest, derived from the evidence which they have afforded on special questions of inquiry. Jupiter is second to none in this respect. At a critical period in the history of astronomy, when the world of science was divided on the subject of the Copernican Theory of the Universe, and when all without the world of science were steadfastly opposed to the new views, the discovery that Jupiter was the centre of a miniature system, circling around him as the theory in dispute taught that the planets circled around the sun, came opportunely as an illustration, and to those who could grasp the significance of the phenomenon, as a proof, of the views of the German astronomer. Later came a yet more remarkable and important discovery, through the observation of Jupiter's system—the discovery that light does not travel, as had been supposed, instantaneously, but with a measurable, however inconceivable, velocity. Through this discovery, supplemented by Bradley's discovery of the aberration of the fixed stars, came a proof—which is absolutely beyond cavil or question—of the true theory of the solar system. Supplementary proofs of Newton's views have been derived also, as might be expected, from the influence exerted by a planet whose disturbing agency so largely exceeds that of all the other members of the solar system.

Let us return to Galileo's discovery of the satellite-system of Jupiter, and the influence of that discovery on the views of astronomers. It was immediately felt, by those who opposed the new views of Copernicus, that the discovery of Jupiter's moons was fatal to their objections. Accordingly they spared no efforts in casting doubts on the observations of Galileo. Some asserted that the Tuscan had seen no such sights as he pretended. Others, that he had indeed seen them, but in illusive dreams; that he was the sport of demons specially sent to punish him for a prying, inquisitive, and truth-doubting spirit. 'We have looked,' they said, 'for hours through his telescope, and have seen no such sights as he and his friends have described.' When at length it was impossible to deny the existence of Jupiter's moons, it became the fashion to dispute the real character of their movements. It was argued that these objects do not revolve around the planet, but travel backwards and forwards behind its disc. Down to the middle of the seventeenth century many refused to believe that the satellites actually circulate around Jupiter.[1]

The discovery, by Cassini, in 1665, that the satellites can be traced when their orbital motions carry them between the planet and the earth, placed the true character of these bodies beyond a doubt. By means of Campani's object-glasses of 100 and 136 feet focal length, Cassini was able to see the satellites projected as small bright spots on the disc of the planet. He found also that their motions when thus situated are precisely those due to an orbital motion around the planet, and therefore very different from those of bodies attached to the planet. This circumstance, and the fact that the bright spots remain unchanged in form as they pass over the disc, proved incontestably that he had not mistaken bright spots, such as are sometimes seen on the body of the planet itself, for the satellites whose ingress on the disc he had previously watched. But he was able to detect another evidence of the true nature of these bodies, since he discovered that the shadows which they cast upon the body of the planet are visible as small dark spots upon the disc.

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[1] For aught I know the motion of the satellites may be denied to the present day. In the preface to the last edition (1823) of the *Principia*, edited by the learned Jesuits Le Sueur and Jacquier, there occurs the following remarkable passage: 'In adopting the theory of the earth's motion, to explain Newton's propositions, we assume another character than our own, for we profess obedience to the decrees of the popes against the motion of the earth.' It is, therefore, not wholly impossible that decrees may have been promulgated against the circulation of Jupiter's satellites also.

Forty years later Maraldi observed that the fourth satellite does not always present the same appearance as it traverses the disc of the planet. Sometimes it appeared to him as a bright spot, at others it appeared darker than the planet. He noticed also that when the satellite seemed to be projected as a dark spot, this spot was smaller than the shadow of the satellite. 'According to the laws of optics,' he says, and others have followed him in the statement, 'it ought to have appeared larger.' Assuming this view to be correct, and that the observations of Maraldi were rightly interpreted by him, we are led to a somewhat singular result. It seems to have been shown by Sir W. Herschel's observations, that all the satellites of Jupiter follow the law observed in the case of our own moon—turning constantly the same face towards their primary. He observed that each satellite varied in brightness in different parts of its orbit, but that when it arrived at the same position in its orbit, 'it exhibits always the same degree of brightness. It would follow from this, that each satellite in transiting the disc of Jupiter should exhibit invariably the same appearance —since when so situated we always see the same half of the satellite, that half namely which is invisible from Jupiter. This, at least, would always happen, unless a satellite were subject to transient variations of brilliancy arising from physical change occurring on its own face. Maraldi's observation would seem therefore to point to the occurrence of such changes on the fourth satellite, and corresponding observations of variations of brilliancy in the other satellites, by Cassini, Maraldi, and Pound, would lead to the same conclusion as respects these bodies also. The observation by Bianchini, that in other parts of their orbits the satellites are subject to considerable variations of brilliancy, would seem to confirm this result.

Now without asserting the impossibility that the above explanation is the true one, I cannot but consider that it is highly improbable that the satellites of Jupiter are actually subject to physical changes of the kind implied. The observations of Sir W. Herschel are decidedly opposed to Bianchini's view, and scarcely less directly contradictory of Maraldi's. It appears to me far more probable that the apparent loss of brilliancy observed by Maraldi was relative only, and due to the projection of the satellite on a brighter part of Jupiter's disc (which we know to be subject to partial variations of brilliancy), than that the whole, or nearly the whole, hemisphere of a satellite should suffer change in the manner imagined. The fact that the satellite appears smaller than the shadow, so far from being contrary to the laws of optics, as many have supposed, is directly deducible from those laws. The black umbra should indeed be smaller, but the complete shadow formed of umbra and penumbra together, should be larger than the satellite.

I may notice, in passing, that observations having reference to the relative brilliancy of celestial objects are at all times difficult, but that those made towards the end of the seventeenth, and in the earlier part of the eighteenth century, appear specially unreliable. Whether from the use of unwieldy focal lengths, or from imperfection in the single object-glasses, or from a want of thorough appreciation of irregularities due to atmospheric causes, certain it is that there are recorded a multitude of observations of this sort in the interval named, which have not been confirmed by subsequent observation.

Soon after his discovery of Jupiter's satellites, Galileo perceived the use to which the phenomena they presented might be applied for the determination of the longitude. He was sanguine indeed as to the use of this method for finding the longitude at sea, not being aware, it would seem, of the mechanical difficulties which render the method unavailable on shipboard. With the object of constructing tables of the satellites' motions, he observed them for many years. The tables he formed disappeared unaccountably on the death of his pupil Rimieri, to whom he had entrusted them for publication, and were accidentally discovered a few years ago in a private library at Rome. Notwithstanding the amount of labour bestowed upon them, the tables are far from representing with accuracy the motions of the satellites. Galileo, indeed, and those who followed him in attempting the work of tabulating these motions, altogether underrated the difficulty of the task. A long series of observations by Hodierna, Borelli, Cassini, Maraldi, Bradley, and a host of other observers; the rigid theoretical scrutiny of the subject by Newton, Walmsley, Euler Bailly, Lagrange, Laplace, and others; and a laborious comparison of the results of observation and theory, by Lalande, Wargentin, Delambre, and Woolhouse, have been required to bring the theory of the system to the exactness and accuracy it has now attained.

The relations actually presented by the motions of the satellites are very singular. They are partly exhibited by the following table :—

Sat.	Sidereal revolution	Same in seconds	Sidereal motion per second	Distance from Jupiter's centre
	d. h. m. s.		″	miles
1	1 18 27 33·505	152853·505	8·478706	278,542
2	3 13 13 42·040	306822·040	4·223947	442,904
3	7 3 42 33·360	618153·360	2·096567	706,714
4	16 16 32 11·271	1441931·271	0·898795	1,242,619

It will be observed, at once, that the period of the second satellite is almost exactly double the period of the first, and the period of the third almost exactly double that of the second; and, of course, a corresponding relation holds amongst the sidereal motions of these bodies. This of itself is remarkable, but far more singular is the relation which regulates the extent to which the above relations differ from exactness. It is to exhibit this that I have added the column of sidereal motions, because the relation in question is masked when the sidereal periods only are given. It will be found that the sidereal motion of the first satellite, together with twice the sidereal motion of the third, is *exactly* equal to three times the sidereal motion of the second satellite. Thus:—

$$(8''{\cdot}478706) + 2\,(2''{\cdot}096567) = 12''{\cdot}671840 = 3\,(4''{\cdot}223947).$$

To show the effect of this singular relation, suppose the first and third satellites to start from conjunction, then after four revolutions of the first satellite, the second has performed nearly one revolution, so that they are very nearly in conjunction again, but have in reality passed their conjunction by a small angle. At the actual moment of conjunction, the first has described three complete circumferences and an arc (A, suppose), which is nearly a complete circumference, while the third has described the arc A only; thus *twice* the motion of the third satellite added to the motion of the first gives us three complete circumferences, and three times the arc A; and therefore by the above relation the second satellite has moved through one complete circumference together with the arc A. Hence neglecting complete circumferences the actual change of position of *each* of the three satellites is the arc A, very nearly equal to a complete circumference. They therefore hold the same relative position at the end as at the beginning of the interval considered. Now nothing was said as to the position of the second satellite. As a matter of fact when the first and third satellites are in conjunction the second is always in opposition to both.

Wargentin, who devoted a life to the examination of the motions of Jupiter's satellites, but who was no adept in the higher branches of mathematics, found as the result of *observation* that the relation above described was so closely approximated to, that 1,317,900 years would have to elapse before the three satellites could be in conjunction. This result affords an interesting measure of the accuracy of observation up to Wargentin's day, since Laplace has shown that the relation is absolutely exact. Librations *may* take place on either side of the mean state (though the most careful modern observations exhibit no trace of such libration), but there is no possibility of accumulative change, save by the influence of effective agencies external to the system. It is somewhat singular that the comet of 1767 and 1779 passed through the middle of Jupiter's system without producing any observable derangement of the mean motions of the satellites—a fact which proves conclusively that the mass of the comet must be small, its density inconceivably minute.

In Ferguson's 'Astronomy' it is stated that the motion of the fourth satellite presents no approach to a relation of commensurability with those of the others. A simple relation exists, however, with a closeness of approximation which is quite remarkable. In fact, throughout the whole solar system there is no relation of commensurability which brings closely following conjunction-lines so near to each other as this does.[1] The relation is this :—three times the period of the fourth satellite is 50d. 1h. 36m. 33·813s., and seven times the period of the third is 50d. 1h. 57m. 53·520s.; the difference, 21m. 19·707s., is less than one-1123rd part of the period of the fourth satellite. Thus when the third satellite has travelled round seven times from a given conjunction-line with the fourth, the fourth has gone round three times and in addition one-1123rd part of a circumference, that is less than 20′, and the third overtakes the fourth before the latter has passed over 15′ more (since 15 : 35 :: 3 : 7). This conjunction-line then is separated from a preceding one (the fourth preceding) by less than 35′. The remarkable relation which causes the 'Great Inequality' of Saturn and Jupiter brings neighbouring conjunction-lines nearly $8\frac{1}{2}°$ apart, a distance fourteen times as great as the above.

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[1] Since the above was written, I have found that some tables of elements of the Saturnian system give such periods to the satellites Dione and Enceladus as to produce a yet closer approach than that of the two satellites of Jupiter whose motions are here discussed.

From the connection between the motions of the first three satellites, it follows of course that the periods of the two inner satellites also approximate to commensurability with the period of the fourth. We have, in fact, fourteen revolutions of the second, or twenty-eight revolutions of the first, nearly equal to three revolutions of the fourth; but the approach is not so close as in the case of the third satellite.

From the relation holding between the motions of the first three satellites it is impossible that all these bodies should be eclipsed at once; but it will be readily seen that at regular intervals all three are in the same straight line with the planet's centre. If this happen when the sun (and therefore the earth, which with reference to Jupiter may always be considered to be close to the sun) is near the same line, these three satellites will be invisible, one or two being eclipsed, two or one (as the case may be) being projected on Jupiter's disc. Such a phenomenon is not unfrequently observable.

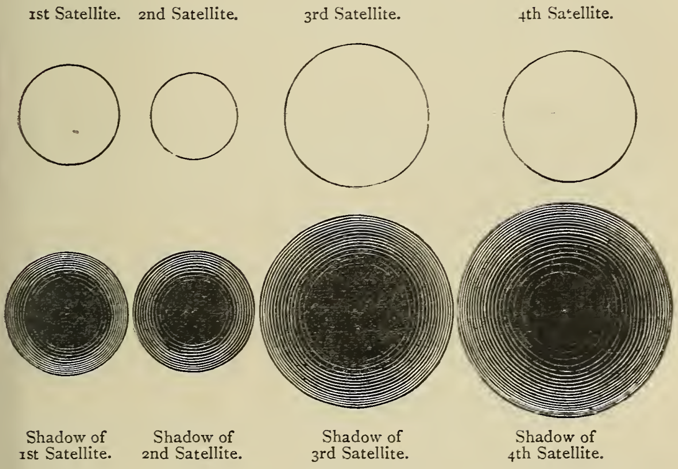
That the fourth satellite may be hidden at the same time it must be nearly in a line with the other three. This relation is not often presented; and, as already stated, the concurrence of this relation with the requisite configuration as respects the sun and earth, is an occurrence very seldom to be observed.

A circumstance that tends to render the simultaneous disappearance of the four satellites more uncommon than it otherwise would be, is the fact that the fourth satellite is not necessarily eclipsed or occulted at each conjunction with Jupiter. It may pass above or below his disc or shadow.

In fact this happens on an average in more than one-third of the revolutions of this satellite. This is ascribed by Sir J. Herschel to the greater inclination of his orbit; but this is not the correct explanation. In fact the inclination of the fourth satellite is at present less than that of any of the others, and the mean value of its inclination is always less than that of the others. The true reason why this satellite so often escapes eclipse is its greater distance from Jupiter.

It is commonly stated that the third satellite cannot possibly escape eclipse or occultation as it passes behind its primary, and must necessarily transit Jupiter's disc when passing before the planet. I find, however, that it is just possible for the third satellite to pass clear of Jupiter's disc in the latter case. A conjunction of many favourable circumstances is, however, required, and the phenomenon must be a very uncommon one—much more so, indeed, than the disappearance of all four satellites. It is necessary that Jupiter should be in opposition when not far from perihelion, at which time it happens (and but for this the phenomenon could never take place) that the earth is at nearly her greatest distance north of the plane of Jupiter's orbit. The satellite's orbit must have its maximum inclination to Jupiter's orbit, and the satellite must also be at its greatest distance from the last-named plane. The other satellites must also be so situated that the third is at its maximum distance from Jupiter; for it is noteworthy that although the orbits of the two interior satellites are described as circular, and that of the third as of small eccentricity, yet these orbits have an *ellipticity* due to the mutual attractions of the satellites. This ellipticity is wholly different from the ellipticity of the planetary orbits. The former is centric, the latter eccentric, the sun being in the focus of each planetary eclipse, while Jupiter is at the centre of the ellipse traversed by the inner satellites.

The figure shows the relative size of the four satellites, according to the best modern measurements, and the theoretical dimensions and character of their shadows. The shadows have not, indeed, been *seen* as here shown, no telescope yet constructed having sufficed, it would seem, to exhibit the penumbral band surrounding the true shadow as a distinct feature. Yet it must not be supposed because the shadows are not seen as here shown that any reasonable doubt can exist as to their true character. The determination of the extent of the penumbra surrounding the true shadow is a matter of very simple calculation. Thus, in the case of the fourth satellite, we have, first, the distance of the satellite from Jupiter's surface equals 1,150,000 miles (in round numbers). Now the sun, as seen from Jupiter, or from any part of his system, subtends an angle of about 6′; and a moment's consideration will show that the width of the penumbral band is determined by supposing two lines to be drawn from a point on the satellite's limb to the planet (and therefore each 1,150,000 miles long), inclined to each other at an angle of 6′, the chord joining the ends of these equal lines gives the required width (technically, the width is the chord of 6′ to radius 1,150,000 miles). From mathematical


1st Satellite.
2nd Satellite.
3rd Satellite.
4th Satellite.
Shadow of 1st Satellite.
Shadow of 2nd Satellite.
Shadow of 3rd Satellite.
Shadow of 4th Satellite.

tables it is readily found that this chord is about 2,000 miles long. This, then, is the width of the penumbra. If the sun were a point, the shadow would, of course, be exactly as large in appearance as the satellite itself, that is, would be a circle about 2,930 miles in diameter. Half the penumbral band lies within, half without such a circle. Hence, the diameter of the true shadow is only 930 miles, and the penumbral band around this being 2,000 miles wide, the diameter of the space covered by umbra and penumbra is 4,930 miles. It is easy to calculate in like manner the figures of the other four shadows. We may thus draw up the following little table :—

Satellites	Distance from Jupiter's surface	Diameter in miles	Width of penumbral band	Diameter of true shadow	Diam. of penumbra
	miles		miles	miles	miles
I.	223,180	2352	390	1962	2742
II.	380,956	2099	665	1434	2764
III.	634,193	3436	1100	2336	4436
IV.	1,148,623	2929	2005	924	4934

These are the dimensions presented in the figure.

But now it is quite clear that although the actual extent of the penumbra must be such as is here shown, yet no telescope could possibly reveal the full extent of the penumbra, since its outer limit passes insensibly into the general illumination of Jupiter's surface. And different telescopes will present the apparent dimensions of the shadow differently. It would, of course, be absolutely impossible to compare the efficiency of one telescope directly with that of another in this respect; because no amount of practice in the use of the micrometer would enable an observer to estimate the apparent width of the shadow with the requisite exactness. But it is not difficult to compare the work of two telescopes in revealing the extent of Jupiter's shadows indirectly. For it is very obvious that a telescope which does not reveal faint shadows well, would exhibit the shadow of the fourth satellite as *perceptibly smaller* than that of the third; whereas a telescope more efficient in this particular respect would show the shadow of the fourth satellite *larger* than that of the third. Or, even if only the fourth satellite and its shadow were transiting the disc, it is obvious that the former telescope would make the shadow seem smaller than the satellite, while the latter would make the shadow larger than the satellite.

It may be remarked here, in passing, that a difference corresponding to that here discussed respecting the satellites' shadows exists in the nature of the shadow of Jupiter at the parts of the cone entered by the several satellites. Thus the disappearance of IV. takes a much longer time than that of I., not *merely* on account of the slow motion of IV. (as is commonly stated), but on account also of the much wider fringing of penumbra in the cross-section of the cone of shadow where IV. enters it, as compared with the fringe in the cross-section where I. enters.

TERRESTRIAL MAGNETISM.

THE study of terrestrial magnetism is gradually assuming an importance which half a century ago few would have thought it could ever attain. It is being brought into intimate relationship with laws affecting not our own earth only, but the whole of the solar system. Indeed, interesting as are the bonds of union which Copernicus and Kepler and Newton have traced in the relations of our system, it would seem as though we were approaching the traces of a yet more wonderful law of association. We see the earth's magnetism responding to the solar influences, not merely in those rhythmic motions which belong to the periodic variations, but in sudden thrills affecting the whole framework of our globe. The magnetic storms which are called into action by such solar disturbances as the one of September, 1859 (witnessed by Messrs. Carrington and Hodgson), are, we may feel sure, not peculiar to our own earth. The other planets feel the same influence—not perhaps in exactly the same way, but according to the constitution and physical habitudes which respectively belong to them. So that one can scarcely conceive a subject of study at once more promising and more interesting than the science on which I now propose to make a few remarks.

To deal fully with terrestrial magnetism would require very much more space than is now at my disposal. The mere history of the science would need a volume; its relations to experimental physics would need another; and one other at least would be required to treat of the theories which are suggested by the interesting relations which have been discovered by modern observers.

In this paper, then, I merely seek to make a few notes on certain points which have occurred to me in reading what Arago, Hansteen, Humboldt, Sabine, and others have said on the subject of terrestrial magnetism. There are certain peculiarities in the way in which the results of observation have been dealt with, which I wish in particular to comment on. I think, too, that certain general results appear to follow from what has been discovered respecting the secular variations of the earth's magnetism, which have not yet (so far as I know) been very closely attended to.

There are three features of the earth's magnetic action which are chiefly attended to by observers—the *declination*, the *inclination*, and the *intensity*.

The declination is the angle at which the horizontal needle is inclined to the north-and-south line. The inclination is the angle at which the dipping needle is inclined to the horizon. The intensity is the magnitude of the force with which the needle seeks the position of rest.

Now if we travelled over the whole surface of our earth, and carefully determined the declination, the inclination, and the intensity of the magnetic action at every point, we should be able to map down on a chart of the earth the relations thus presented to our notice. And speaking generally we should have the following peculiarities to deal with :

First, as to the declination. We should find that in certain regions the magnet's northern end was to the west of north, while in certain other regions the reverse was the case. If we marked in the boundary line between these regions, it is obvious that we should have traced in a line along which the needle would lie due north and south. This is what is termed a line of no declination. On charts of the earth's magnetic relations, the position of this line for about the middle of the present century is usually indicated. In some maps a set of lines used to be added, along each of which the magnetic needle had a definite declination.

No such lines are now marked in, however. The reason why they are omitted is that they are very complex. But I wish to call particular attention to the omission, for this reason, that the same consideration which renders the lines of equal declination unnecessary in a chart of the earth's magnetism, the same reason which gives them a complexity altogether unmeaning so far as the cosmical relations of terrestrial magnetism are concerned, renders also the line of no declination worthy of much less consideration than it has received at the hands of many eminent physicists. I shall presently show why this is so.

Secondly, as to the inclination. We should find as we travelled over the earth's surface, that the dipping needle tends to verticality at two nearly opposite points, one close to the Arctic and the other to the Antarctic circle. These are called the northern and southern inclination-poles, and must not be confounded with the intensity-poles presently to be mentioned. As we leave either inclination-pole the dipping needle leaves its vertical position, and gradually approaches the horizontal direction, until, along a curve lying midway between the two poles, the needle becomes exactly horizontal. This curve is called the magnetic inclination-equator. The present positions of the inclination-poles are indicated in magnetic maps. The inclination-equator is also indicated. The curves which run like parallels around the two poles in such maps indicate the curves along which the dipping needle has a definite inclination (different, of course, for each curve). Another series of lines, which intersect in the poles, or at least tend towards the poles, run so as to cross the inclination-parallels at right angles. But they illustrate, properly speaking, the peculiarities of declination. They show in what direction the horizontal needle points in different parts of the earth. But it is to be noticed that as they cross the real meridians at constantly varying angles they are in no way associated with the lines of equal declination described in the preceding paragraph.

Lastly, as to the intensity. If we noticed, in every part of the earth's surface, the number of times the needle vibrated through its position of rest in a given interval (this number affording a very exact measure of the intensity of the magnetic directive action), we should find that along a curve lying near to, but not absolutely coincident with the inclination-equator, the intensity has a minimum value. This curve is called the intensity-equator. Leaving it either towards the north or south, we should find the intensity gradually increasing. We should not, however, find this increase guiding us to a northern or southern intensity-pole; but we should recognise two magnetic intensity-poles in each hemisphere. The positions of these are indicated in magnetic maps. The intensity-equator shows a certain resemblance to the inclination-equator, but the two equators do not coincide.

Now in considering the various relations here presented, it is very important that we should decide which property of the magnetic needle should receive our chief attention.

General Sabine considers that the intensity is the primary quality of the magnet in all such inquiries as we are at present concerned with; that is, in all inquiries respecting the wider relations of terrestrial magnetism. He remarks that 'all that relates to the *force* has a more immediate bearing than what relates to the *direction* of the needle, either in the horizontal or vertical plane; these planes, although necessarily used by us both in observation and discussion, not bearing in themselves any direct relation to magnetism.'

Without denying the force of these remarks, I could yet venture to point out reasons why the force is a less suitable relation than the direction, where the question is one of mapping down the equator, parallels, and poles, which are to teach us something of the earth's magnetic action and its secular changes.

I must premise that the declination and the inclination may be combined in a single indication—viz., the *direction*, with a certain convenience so far as the general teachings of terrestrial magnetism are concerned; but that if we are to select one or other of these elements of the direction as our special guide, it must clearly be the inclination. For a glance at a map of the earth's magnetic relations will show that over all the earth, except those regions which lie close to the poles, the declination has comparatively narrow limits of range, whereas the inclination varies from 0° to 90°.

Now the great point to be determined by the student of terrestrial magnetism is the situation of the true magnetic equator and poles. The existence of more poles than one in each hemisphere is clearly a peculiarity depending on the irregular conformation of the earth's frame, combined—in some way as yet unknown—with the external influences on which the primary facts of terrestrial magnetism depend. We cannot doubt that if the earth were a homogeneous sphere she would have a definite magnetic axis and a definite magnetic equator, which would be the same in position, whether we considered them with reference to the intensity or the directive power of the needle. And what we must do if we are to learn what position the magnetic axis and equator would have, *but* for terrestrial irregularities, is to select that relation for our guidance which is least likely to be affected by such irregularities.

Now I shall be able, I think, to show that the inclination has a very great advantage over the intensity in this respect. The matter is very simple, and has as yet, as it seems to me, been very little considered.

How do we determine the intensity-equator and the intensity-poles? The answer is, by determining whether the intensity is a maximum or a minimum. Now, it is a well-known principle that variable quantities change slowly in the neighbourhood either of a maximum or of a minimum;[1]

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[1] We see an instance of this in the change of the sun's midday elevation near the time of midsummer and midwinter; and many other equally familiar illustrations will occur to the reader.

so that if we have to determine the position either of a point where the intensity has a maximum or minimum value, we might easily make a very appreciable mistake, merely from the fact that there is so slow a rate of change near such a point, and that small irregularities due to local peculiarities may easily set us far astray.

The fact, then, that the intensity-poles and the intensity-equator are found by the determination of a maximum or minimum is very unfavourable to the choice of this relation, intensity, as our guiding element. It is as though an astronomer were required to determine the seasons of the year, not by observing the sun's passage of the vernal or autumnal equinox (which is comparatively easy, because the sun crosses the equator at a considerable angle), but by noticing when the sun attained his greatest distance above or below the equator, which is not easy because his distance from the equator changes so very slowly at those times, when, in fact, his motion is all but parallel to the equator.

Let us see if there is any difference when we consider the inclination.

The inclination-poles and equator are determined by noticing where the dip is 90° and 0° respectively. Now the dip changes *through* these values as sharply as it changes through any other values. In other words, a change of place on either side of the line of no dip, or on either side of the place of the vertical needle, produces quite as much change in the inclination as a similar change of place from any of the inclination-parallels. The fact is, that if we could travel from one of the inclination-poles to the inclination-equator, thence to the other pole, and so on until a complete circle of the earth had been made, we should find the dip changing from 90° down to 0°, thence to 90°, and so through 0° to 90° again, and always at the same rate, precisely as the elevation of the pole of the heavens would change if we could make the circuit of a meridian.

It seems to me that for these reasons the inclination of the magnetic needle is an element whose indications are far less likely to mislead us than those of the intensity. In fact, it appears almost as unreasonable to be guided by the intensity in determining the true magnetic equator, as to endeavour to determine the position of the earth's equator by a reference to the distance of different points on the earth's surface from the centre of our globe. Doubtless, a curve approaching the earth's equator *pretty nearly* might, with much labour, be determined in this way; but the determination by a reference to the elevation of the celestial pole is not likely to be abandoned by astronomers on that account.

Now, having determined that the directive power of the needle is the guide which can best lead us to the determination of the true magnetic equator and poles, we must yet make some remarks on the two elements of the magnet's direction.

A stress which appears to me altogether unreasonable has been laid on the position and motions of the line of no declination. In every work on terrestrial magnetism with which I am acquainted, this line is made the subject of a long disquisition. Humboldt, Arago, Hansteen, and Sabine discuss its present peculiarities, and the peculiarities of those motions by which it has reached its present position. In fact, in endeavouring to form a general idea of the laws which govern terrestrial magnetism, these and other eminent physicists have paid more attention to the motions and peculiarities of this line than to any other feature.

I would ask whether the line of no declination really merits such close attention; whether rather it is not, of all the curves associated with magnetic mapping, the one most likely to lead us astray. And to prepare the reader to pay closer attention to this point than in the face of so many eminent authorities he might care to do, I may just mention the fact that (whether the line of no declination is *calculated* or not to lead us astray) it actually has led physicists to form directly opposite conclusions on a matter of extreme simplicity. The most striking feature of the line is its rapid change of position, and yet physicists cannot agree which way it is travelling. Arago says it is clearly travelling from east to west. On this Sabine remarks that quite obviously the line is travelling from west to east, and he quotes Hansteen's maps in confirmation ; but Humboldt, who had carefully studied Hansteen's maps, takes the same view as Arago.

The real fact is, that the choice of 'the line of no declination' as a guide to the real history of magnetic changes, is as unfortunate as the choice of the intensity as a guide to the position of the true magnetic equator and poles ; and for much the same reason. The slightest irregularities due to the effects of the configuration of different parts of the earth's surface suffice to cause the line of no declination to assume the most complex and irregular figures. And again, the grand process of cosmical action which leads to a continual change in the position of the magnetic equator, causes the true line of no declination (that is, the line along which, if the earth were a perfectly uniform globe, there would be no declination) to be continually subjected to new irregularities, which have no bearing whatever on the cosmical laws of terrestrial magnetism.

Let me illustrate my meaning by two examples :—

What is required, that a point should be on 'the line of no declination,' is that the magnetic direction-lines should there be running north and south. Now suppose that on a certain portion of the earth the direction-lines have the position of slant-waved lines only running north and south near their middle points. Then a curve through these points belongs to the line of no declination. But now conceive that, through some cause or other (as a change in the earth's condition in this neighbourhood), the 'lines are slightly twisted, so that at these points they do not run north and south but nearer the general direction of the waved lines. Then there are no points at all belonging to the line of no declination, within the area covered by the lines. Thus the portion which had crossed this area disappears altogether, merely through the effects of a very slight change.

Next consider a case where a set of direction-lines have no points of inflection, but become north and south without change of curvature. Suppose that here, also, a slight change is produced on the position of the direction-lines by local irregularities, and that this change produces such a change of curvature where the convexity of the lines had touched the north and south lines. Then, in place of a single line, we should have three lines of no declination across this region of the earth.

What reliance, then, can be placed on the indications of a curve which is liable to such remarkable changes of figure, through disturbances quite within the limits ascribable to the effects of local irregularities in the magnetic action? For it is to be remembered that the cases I have cited are not instances carefully selected to exhibit, in a magnified form, the effects of local irregularity. The direction-lines can only assume a north-and-south position in one of the two ways considered; that is, either by inflection or by presenting their convexity to the terrestrial meridians. Nor are such disturbances as I have considered by any means exceptional. On the contrary, everything tends to show that at every point of the earth's surface the main effects due to the earth's magnetic action are slightly modified by local irregularities. If a plan had been wanted for losing sight of the main effects of magnetic action, no better one could have been thought of than that of selecting the 'line of no declination' as a guide. One might as well attach importance to a line on the earth, showing where gravity acts in an exactly vertical direction. We know that, owing to local irregularities, we should find such a line to be looped and twisted into curves of the strangest complexity, but no one has ever thought that those curves could teach us any great truths respecting gravity.

The reader will gather from the above remarks that my chief aim in these notes is to draw attention to the more marked characteristics of terrestrial magnetism. Any chart of the earth's magnetic relations will serve to illustrate what remains to be said; but stereographic charts [1] of the northern and southern hemispheres are the best. The irregularly curved link with its companion loop, which forms the 'line of no declination,' may be left out of consideration. The inclination-equator, the series of lines and parallels belonging to the same element of the magnetic action, and the poles towards which these lines tend, or around which they lie, are to be our principal guide.

The magnetic poles are not directly opposite each other. A point directly opposite the estimated place of the southern magnetic pole (Ross was unable to reach this point) would lie to the west of the northern magnetic pole, and nearer the true pole of the earth. And it is worth noticing that many of the direction-lines around the northern pole seem to indicate a point further west as that to which they really tend. All those which cross the northern parts of the Pacific exhibit such a tendency in a very marked manner. This is a circumstance of which I shall presently have occasion to remind the reader.

The magnetic equator exhibits a tolerably uniform sweep, but it is not a perfect circle. It will be noticed that where it crosses the ocean it seems to seek a more northern course.

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[1] My reason for preferring this projection in the present instance is that it presents correctly all the angles at which the various lines on the earth's surface intersect each other. This is important in the case of magnetic charts. Otherwise I have no great liking for the stereographic projection. In my *Handbook of the Stars*, I have pointed out the incorrectness of the view that this projection is suited for exhibiting the earth in hemispheres, remarking that however truly the projection exhibits small figures, it cannot (as alleged) exhibit continents without great distortion. A glance at Asia and at South America, as exhibited in such stereographic charts as I have suggested, will serve to show that this view is just. It is important, however, to notice one great advantage of the projection, for the purpose mentioned. All circles on the sphere are projected into circles, so that where we see the magnetic parallel somewhat oval in figure, we know that they really do depart from the circular form.

Next as to the parallels. It seems worth noticing that as a general rule the neighbourhood of the great continents seems to sway these curves away from the northern magnetic pole. The parallels in fact assume in the northern hemisphere an oval figure, the direction of the major axis lying along a meridian through Asia and America. The way in which the parallel nearest the southern pole is swayed northwards where it crosses the Atlantic Ocean is also well worth noticing.

These irregularities are neither marked enough in their character, however, nor consistent enough in their indications to detain us here. Nor need anything be said about the peculiarities of the magnetic direction-lines (or meridians, as they have been somewhat strangely named).

Now let us remember what these lines and parallels represent. When we see London and New York nearly on the same magnetic parallel, we learn that the inclination of the dipping needle is nearly the same in London as in New York. If we go south from either place the inclination diminishes, if we go north it increases.

Suppose, then, we find, that as time passes the inclination in London or in New York is changing—what opinion are we to form? We can arrive at no other conclusion than that the distance of these places from the magnetic pole is changing. And since London and New York are not changing their place on the globe, we conclude that the magnetic pole is in motion.

But the inclination in London *is* changing, is in fact steadily diminishing. We can at once interpret this change. Since by travelling away from the magnetic pole, we discover a continually diminishing inclination, England must be receding from the nearest magnetic pole; in other words the northern magnetic pole is receding from England. We see at once that the magnetic pole must be travelling westward.

But now let us adopt a new mode of discussing this important secular variation.

One of the most thoroughly established of all the peculiarities of terrestrial magnetism is the fact that the magnetic declination varies from time to time. In London and Paris the variations of the declination have been observed more carefully, and for a longer time, than at any other place. Let us inquire what we can learn from them.

They may be briefly stated thus:—

In London the needle pointed to the east of north before the year 1657, when it pointed due north. From that time the westerly declination gradually increased, until the beginning of the present century, when the westerly motion was observed to flag. In 1819, the greatest westerly declination was reached. At this time the needle pointed 24⅔ degrees to the west of north. Since then the needle has been slowly travelling eastwards, and the westerly declination is now only some 17 degrees, the needle pointing now as in fig. 1.

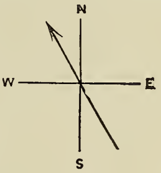


Fig. 1.

In Paris the needle pointed due north in 1663. Its subsequent motions have closely resembled those of the London needle; but the Paris needle ceased to move westwards as early as 1817, and the greatest westerly declination attained by it was only 22½ degrees.

Now we can easily interpret both these sets of movements. If we were to perplex ourselves by looking at the line of no declination, to see where the needle points due north, we might indeed go very far wrong. But if we seek for direction-lines, which have a *general* north and south direction, we see that these lines lie nearly on the great circle passing through the northern magnetic pole and the pole of the earth. It follows that at some time near the year 1657 the northern magnetic pole must have been on the meridian of Greenwich, or on the prolongation of this meridian forming the meridian 180° E. (or W.) of Greenwich.

In other words the magnetic pole must either have been directly between England and the real pole of the earth, or directly beyond the pole.

To determine which of these positions the magnetic pole had is very easy. If this pole were between England and the real pole of the earth, it is obvious that the magnetic inclination in England would have had a maximum value. In the other case the inclination would have had a minimum value. But the inclination has been continually diminishing since 1657. It is now as shown in fig. 2. Hence it then had a maximum value; and the northern magnetic pole lay between England and the real pole of the earth.

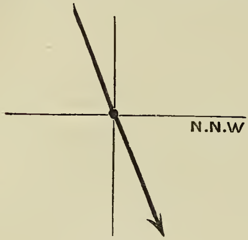


Fig. 2.

Also we can tell which way the magnetic pole was travelling. For before 1657 the declination was easterly, whereas afterwards it was westerly. Hence the magnetic pole must have travelled from east to west round the north pole of the earth. This agrees with our former result.

We can learn also something about the rate at which the magnetic pole is travelling.

Supposing the magnetic needle in the meridian of London in 1657, and Ross's estimate of the place of the magnetic pole to be appreciably correct, giving (in round figures) 95° W. of Greenwich for the longitude of the magnetic pole in 1833, we get a period of revolution of

$$\frac{360}{15} \times (1833-1657) \text{ years}$$
$$=\frac{72}{19} \times 176 \text{ years}=667 \text{ years about.}$$

Combining Ross's estimate with the Paris epoch, we get a period of

$$\frac{72}{19} \times (1833-1663) \text{ years}$$
$$=\frac{72}{19}(\times 170) \text{ years}=644 \text{ years about.}$$

The mean of these values is about 655 years, and I think there is very good reason for believing that the northern magnetic pole revolves around the north pole of the earth from east to west, in about this time.

The southern magnetic pole must, of course, have the same period of rotation as the northern. Hence a portion of the discrepancy alluded to above, between the northern magnetic pole and the antipodes of the southern, as estimated by Ross, is accounted for; because Captain Ross determined the position of the northern pole ten years before he endeavoured to reach the southern one, and in ten years the northern pole had rotated over a considerable arc towards the west.

The magnetic equator must also vary in position. Its inclination to the real equator may perhaps be nearly constant, but its nodes must travel round from east to west, making a complete revolution in the same period as the poles. Such a motion has been actually observed.

I am sensible that when we examine the changes of the declination and inclination, over the whole face of the earth, so far as they have hitherto been determined, we shall find many irregularities which seem, at first sight, not easily reconciled with the above views. But it will be found that it is only when weight is placed on the relatively unsatisfactory indications already mentioned that these peculiarities become perplexing. Three circumstances which have been much dwelt upon as opposed to the existence of such a rotation of the magnetic poles as I have described remain to be mentioned as among the clearest evidences that that rotation is actually taking place. Over the Chinese empire, Humboldt says, the change in the magnetic declination has been very slow indeed for several centuries. In Jamaica, Sir J. Herschel notices the needle has pointed nearly due north for many years; and lastly Arago notices a similar peculiarity in the case of South Australia. Now if we look at a chart of the earth's magnetic relations, we shall see that the reason of these peculiarities is closely bound up with the rotation I have described. The northern magnetic pole in passing from the meridian of Greenwich to its present position, has passed the point where it has its greatest distance from the Chinese empire, and consequently the oscillation of the needle (by the reverse of what was inferred for the case of London) would be slowest during the centuries occupied by this passage. In Jamaica and Australia the neighbouring magnetic pole lies nearly due north and due south respectively, and has lately passed the meridian of those places, so that here, as in England in the middle of the seventeenth century, the needle has for many years had a small declination.

THE STAR-DEPTHS.

During the last two years it chances that I have many times given lectures on the stars, and, accordingly, it will not be wondered at that many suggestive questions have been addressed to me by thoughtful persons to whom the wonderful teachings of modern astronomy respecting the star-depths have been submitted. In particular—and this is one circumstance in which, as I conceive, the lecturer has an advantage over the author—I have had an opportunity of ascertaining the difficulties which have occurred to those who have heard the narrative of what astronomy has done. I propose in the present essay to consider some of the points which have thus been brought under my notice, feeling assured that a large proportion of those who study astronomical subjects are likely to find an interest in the matters I am now to touch upon.

I may remark, in the first instance, that misapprehensions are somewhat widely prevalent as to the knowledge which astronomers have obtained respecting the distances and real magnitudes of the fixed stars. The announcement is received with astonishment, and some degree of disappointment, that astronomers have only estimated the distance of a single star in the whole heavens in a manner which can be regarded as satisfactory ; while even that estimate is probably many hundreds of millions of miles in error. Many appear to imagine that it is in a sense discreditable to astronomy that no greater success has been achieved in this direction ; others suppose that the statement has its origin in doubts or objections peculiar to myself; while some recall the fact that in books of astronomy the estimated distances of ten or twelve stars are indicated, together with a variety of elaborate calculations as to the average distances of stars of the various orders of apparent brightness, or (as it is technically termed) *magnitude.*

It is well that it should be generally known how powerless astronomy at present is in this matter of the determination of stellar distances. The astronomer can deal only with the actual conditions of a problem of this sort; he cannot select the conditions for himself. The surveyor of a difficult region of the earth can determine heights and distances by a variety of methods; he can undertake and execute a variety of more or less difficult operations; and where one process of survey does not satisfy him, he can try another, or several others. But the astronomer who wishes to determine the distance of a star, has one means only for obtaining that change of standpoint on which all such measurements must, in the long run, depend. And that means does not depend upon himself. It is not *he* who is to undertake the journey by which the change of standpoint is to be obtained, but the earth carrying him and his telescope along with her in her yearly orbital motion round the sun, shifts his standpoint for him. The case is even less under his control than the problem of determining the sun's distance, now so engrossing the attention of astronomers; for there the astronomer may vary his standpoint more or less, according as he is more or less in earnest in seeking to master his difficulties. He may, on the one hand, seek the bleakest and most dismal northerly regions, as the Russians propose to do during the coming transit of Venus; or, on the other, he may be careless in pushing his way as far southwards as possible, even as (it is to be feared) this country is likely to be careless, on that important occasion. But the measurer of a star's distance has no choice in the matter. He must be content to wait until the earth has carried him from one side of her path to the other, a span wide enough, it should seem, to measure almost infinite distances, since the base line thus given is more than 180 millions of miles in length. But it is all too narrow for measuring star-distances, and if the astronomer could choose he would require the orbital sweep of Saturn, Uranus, and more distant Neptune, to give him a really effective hold on the stupendous problem. Even then, however, he would be left powerless in the presence of all but a few hundred stars ; beyond these, the millions on millions known to astronomers would lie at distances which would laugh to scorn all means of measurement. In fine, to measure effectually the distances of the stars, the astronomer should be able to voyage with an interstellar comet, and live millions of years for every hour of his present life.

To show how feeble is the actual hold of the astronomer on this great problem, let us consider what the base-line of 180 millions of miles really represents, in the case even of the nearest of all the fixed stars. To the observer, when stationed at one end of this vast line, a star is seen necessarily in a different direction than to the same observer when (six months later) he is at the other end of that line. But the two direction-lines are so nearly parallel, even in the case of the nearest star, that if two lines were drawn as nearly parallel from a point, they would have to be extended to a distance of nearly two miles from that point before they would be a single inch apart. Or the matter may be presented in this way. We know how little the minute hand of a watch or clock changes in direction in a single second of time. Now the direction of a line from the nearest star in the heavens to the earth shifts during the half-year by less than the change of direction of the minute-hand of a watch or clock in the two-hundredth part of a second. This is as much as to say that if that star be regarded as the centre of a monstrous clock-face, and a line from the star to the earth as the minute-hand, then the extremity of that hand would move over 180 millions of miles in the two-hundredth part of a second of time.

Now if astronomers had merely to recognise this change of direction, the task must be a sufficiently arduous one. In fact, two centuries passed after the first use of telescopic measuring-means before the slightest signs of displacement could be recognised in any star in the heavens. The earth during all these years was circling on her orbit with its vast span of 180 millions of miles: and yet the star-sphere within which this motion was taking place remained seemingly as unchanged as though the earth had been absolutely at rest—and this although the most powerful instrumental means invented by man were applied to detect even the minutest sign of motion. But, as a matter of fact, the astronomer has to accomplish a much more difficult task before the distance of a star can be ascertained. All that the mere recognition of displacement would imply would be that the star does not lie at a distance so vast as to reduce to absolute nothingness the effects of the earth's orbital motion. The displacement must not be *recognised* only, but *measured*, before a star's distance can be estimated. Thus I have spoken of the change of direction as nearly equal to the two-hundredth part of the motion of the minute-hand of a clock or watch in a single second of time. It is such a fact as this that has to be ascertained. The astronomer must be able to say that the change is, let us say, greater than the two-hundred-and-tenth part and less than the two-hundredth part of such motion of a minute-hand, or to make some other definite statement indicating the limits of error in the result of his observations. Then, and then only, the star's distance can be said to be estimated; though even then, be it noticed, the amount of error in the determination of a star's distance remains necessarily enormous. In the imagined case the error is probably within the twentieth part of the star's actual distance, but the twentieth part of the distance of the nearest star is more than ten thousand times the sun's distance.

After this it will not be regarded as very surprising that I reiterate the statement made above to the effect that the distance of only one star has been determined satisfactorily. But I will now adduce independent evidence on this point. The nearest star in the whole heavens is the leading brilliant, Alpha, in the Centaur, a star not visible in our northern latitudes. Now the next in order of distance (always referring, of course, to the observations made thus far) is a faint star in the northern heavens, numbered 61 in the constellation of the Swan. Until of late it has been judged that this star lies about three times as far away as Alpha in the Centaur, and this estimate has been for many years repeated in our text-books of astronomy. But recently a more careful and complete series of observations has been effected, with the unexpected result that, so far as these particular observations are concerned, the distance of this star must be set at twice instead of three times the distance of Alpha in the Centaur. Of course, this result, though it must be regarded as the most satisfactory yet obtained, cannot be regarded as final. The mere fact that it differs from the result so long accepted shows how open the whole subject is to doubt. And then, when we consider what the difference between the two results actually amounts to, we see that the very errors in the attempts to determine star-distances belong to the same order as the distances themselves. Here, in the case of the nearest but one of all the stars in the heavens, we find that one estimate of the star's distance exceeds another by a full distance of the nearest star, or by upwards of two hundred thousand times the sun's distance. After this, it appears to me that it would be preposterous to place reliance on the estimates of the distances of stars yet farther away. Astronomers have made one sure step, and stumbled at the second. Can it be thought that in the steps beyond the second they have walked surely?

But so soon as we recognise the fact that of all the millions of stars, one only has had its distance satisfactorily determined, while perhaps some ten or twelve lie at measurable distances, we see that all our conceptions respecting the star-depths must be, to a great extent, hypothetical. On our ideas of star-distances depend our ideas of star-magnitudes. Or rather, we cannot even go so far as this; since it would only be possible to estimate the real magnitudes of the stars if we could measure their apparent magnitude. But so enormous are the distances of all the stars, that even with the most powerful telescope the largest and brightest stars remain mere points of light. So that, in fact, all we can do to form an idea of the real magnitude of any star is to compare the quantity of light we receive from the star with that which we receive from our own star, the sun. This has been done, and the general result is important and interesting. It is this: if our sun were placed at the distance of the nearest of all the stars, he would shine as a star of the second magnitude (just bordering, however, on the class of first-magnitude stars). Now all the observations hitherto made indicate that the first-magnitude stars lie, on the average, fully five times as far from us as the nearest star Alpha in the Centaur. At this distance, the sun would shine only as a star of the fifth magnitude—that is, only an order higher than the very faintest stars seen on a perfectly clear and dark night. The inference manifestly is, that our sun is very much smaller than the majority of the stars which form our constellations.

I pass now to another point, on which many misapprehensions prevail. I have frequently been asked what, in my opinion, is the actual form of our star-system regarded as a whole. It is known that various hypotheses have been formed on the subject, and my own writings sufficiently indicate that I hold opinions which are, at least to this extent, definite—that they are opposed to those commonly presented in our text-books of astronomy. But what is not uncommonly asked is, that I should indicate what is the actual figure I prefer to the cloven flat disc of our textbooks.

Now I think it will be tolerably clear, from what has been said respecting star-distances, that our ideas as to the figure of our star-system must necessarily be hypothetical, even if not speculative. Since when we look at any star, save nine or ten out of the millions revealed by the telescope, we are in absolute ignorance as to the star's distance, we must necessarily be ignorant as to the shape of the system which those millions of stars form, when regarded as a whole.

It will not be marvelled at, then, if my reply to the question just mentioned is, that in my opinion the figure of our star-system is unknown, and in all probability unknowable.

Where then, it may be asked, do my views respecting the sidereal universe distinctly clash with those commonly (at least until quite recent times) received amongst astronomers? Or, do they merely differ in this respect—that I regard as unsupported by sufficient evidence what has commonly been held to have been placed on a highly probable basis?

I will indicate as plainly as possible in what respect my views about the sidereal universe, and the methods of inquiry I have adopted or suggest for future use, differ from the views and methods of those who have preceded me in this department of research. To make the matter simpler I shall limit my remarks to the views and methods of Sir W. Herschel and the elder Struve, because these astronomers alone have advanced, or dealt with, wide and general considerations.

Every reader of astronomical text-books is probably familiar with Sir W. Herschel's method of observation called star-gauging, and with the result which he obtained from it in 1785. Oddly enough—the mistake is however of a piece with others made in this matter—the paper of 1784, in which Herschel described the method, is selected by compilers in preference to the papers of 1785, in which he described his application of the method and its results. Struve, alone, so far as I know, has pointed out the absurdity of this process. As he justly remarks, the later paper supersedes the earlier altogether. Yet Arago, Guillemin, and all the French writers of astronomy, and of course their English followers, have complacently reproduced the preliminary statements of 1784, as though indicating Herschel's conclusions from the method of star-gauging.

The method of star-gauging assumes such a degree of uniformity in stellar distribution within our star system—assumed to have attainable limits—that the number of stars seen within any given field indicates fairly the extension of the system in the direction of that field. Taking fields equal in size and observing them with equal telescopic light-gathering power, we should have the same sort of estimate of extension that we should obtain of sea-depth by letting down a tube to the sea-bottom and weighing all the water inclosed within the tube. All that is necessary to make the estimate satisfactory is that we should really reach the limits of the system, and that the stars really should be spread with tolerable uniformity. The size of individual stars counts for nothing in this method, assuming only (as Herschel assumed) that a telescope powerful enough to reach easily to the limits of the system would show every star within the system; in other words, that there are no *very* great discrepancies of magnitude.

It was as the result of this process and no other that Herschel (following Wright, of Durham, in this respect) arrived at the well-known stratum theory of the sidereal system—or rather the cloven stratum theory. This theory, plainly expressed, is as follows: our sidereal system is flattened in shape, limited in extension, cloven into two strata through about one half of its extent, and star-strewn throughout its whole extent. The stars of the Milky Way, according to this theory, constituted the main part of the sidereal universe.

When this theory was formed, Herschel had not met with even the simplest cases of unequal star-distribution; he had not yet learned that such objects as binary stars exist in the universe. Struve has well pointed out that the discovery of the physical association between certain double stars changed the whole aspect of the star system in Herschel's eyes. From this time he recognised the fact that there are different orders of stars, and different systems of star-grouping.

Does it need proof that Herschel himself abandoned altogether, as well the principle of star-gauging as the results to which it had led him? It ought not, but as a matter of fact it does require proof; it requires reiterated proof; it requires to be stated and re-stated, and demonstrated and re-demonstrated, until the writers of astronomical text-books shall begin to entertain the idea that possibly the theory they advance so confidently as Herschel's may have been doubted by Herschel as time went on.

Here are his words in 1802—they seem clear enough: 'The stars we consider as insulated are also surrounded by a magnificent collection of innumerable stars called the Milky Way. For though our sun and all the stars we see may truly be said to be in the plane of the Milky Way, yet I am now convinced by a long inspection and continued examination of it, that the Milky Way itself consists of stars very differently scattered from those which are immediately about us.'

I only select these words because they afford such simple and conclusive evidence of Herschel's change of opinion. There are even more convincing details, however, which, to be properly understood, require to be examined in their proper place in Herschel's series of papers. Herschel not only said that the Milky Way was not what he had supposed; but he stated what in his opinion was the actual shape and constitution of certain parts of the Milky Way. He found that certain of the brighter parts of the Milky Way were not regions of the sidereal system owing their apparent richness to their vast extension, but true clustering aggregations—not parts of an extended stratum, but globe-shaped. He says of these regions, 'we may, indeed, partly ascribe the increase both of brightness and of apparent compression to a greater depth of the space which contains these stars; but this will equally tend to show their clustering condition; for since the increase of brightness is gradual, the space containing the clustering stars must tend to a spherical form, if the gradual increase of brightness is to be explained by the situation of the stars.'

In 1811, he used the following expressions: 'When the novelty of the subject is considered, we cannot be surprised that many things, formerly taken for granted, should, on examination, prove to be different from what they were generally (that is, in a general sense, not by the generality of persons) but incautiously supposed to be. For instance, an equal scattering of the stars may be admitted in certain calculations; but when we examine the Milky Way, or the closely compressed clusters of stars, this supposed equality of scattering must be given up.'

Herschel did not substitute a new method of stellar observation, or indicate any definite new views respecting the sidereal universe, until a much later epoch in his career.

It was in 1817 that Herschel described his *resolution-test*, very commonly confounded with star-gauging, but quite distinct in principle. According to this plan the distance of any star-group was to be determined by the telescopic power necessary to resolve the group. To show the distinction between this method and the other, it is only necessary to point out that in star-gauging, the same telescope, or at least the same telescopic power, was necessarily employed throughout the inquiry; in the new method, the telescopic power was made variable, gradually increasing power being applied until a star-group was completely resolved, and the power required for this purpose was held to indicate the distance of the group. There was a partial return, in the adoption of this method, to the general idea of uniformity of distribution which had led Herschel to erroneous results in 1785; and in precise correspondence with this fault in the new method we recognise in the results obtained an obvious incorrectness, which would not have escaped the attention of Herschel in earlier years. In fact, I have not the slightest hesitation in saying that, in my belief, the celebrated papers of 1817 and 1818, despite their undoubted power, are yet characterised by signs of the advanced age which Herschel had attained. There is no want of power, but there is not the old elasticity of mind ; 'the grace and versatility of the man' which had been so marked in former years, can no longer be recognised, except in the fact that views so noble should have been conceived at all, and a method so difficult carried out with something of the energy of Herschel's earlier days. It is not, I conceive, to be wondered at if, at the age of seventy-nine, the great astronomer should not display those qualities of mind which specially characterised him in youth and middle age ; and it should not be regarded as indicating any want of respect for the memory of Herschel to point out in what circumstances these latest papers of his appear wanting.

I would note, then, that the new method was one to be tested, not adopted. The method of star-gauging had been adopted, and had eventually to be rejected. This experience should have suggested that no new method ought to be adopted until it has been subjected to some adequate test. The results obtained by Herschel showed sufficiently (and in his earlier years would have been regarded as showing) that the method was unsound.

Let one point suffice to show this :—

The fine double-cluster in the sword-hand of Perseus was partly resolved with the lowest power Herschel employed, and this, according to the criterion he had adopted, showed that part of the cluster lay only just beyond the limits of naked-eye vision. But he was unable with the highest powers he employed to resolve this spot completely, and this, according to his criterion, showed that part of the cluster lay beyond the range of his highest powers, or some twenty times farther away than the limits of naked-eye vision. Thus, according to his criterion, the distances of the nearest and farthest parts of the cluster are as 1 to 20, at least. But the apparent size of the cluster shows that the breadth of any part of the cluster is less than the hundredth part of the distance of that part. Combining these indications, we have for the shape of the space within which the cluster is included something like A B (fig. 1), where S is the place of the solar system.

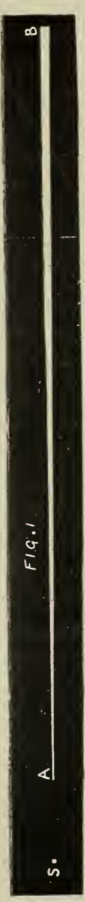


FIG. 1

But no one can accept so monstrous a result as this ; nor would Herschel for a moment have adopted it, had he noticed that his own figures led directly to it. Apart from all other objections, the circumstance that this long projecting cluster had the sun directly at its apex, would have sufficed to show that some other interpretation must be adopted. But no one who has observed this or any other star-cluster can doubt for an instant that the portion of space occupied by the cluster is rounded—speaking roughly. The cluster is not a globular cluster, of course ; it has outlying branches and streams (and appears, by the way, to be connected with the general stream of the Milky Way in this neighbourhood) ; but that, regarding it as a whole, we must consider it as a rounded nodule or double nodule of the galactic stream, and not as a projecting region directed exactly from the sun, no one can doubt who has ever examined the cluster.

Nor did Herschel question this, or fail to recognise the soundness of the method of reasoning, illustrated by Fig. 1, which I have used to dispose of the strange result to which Herschel's figures lead. But the real fact is, that Herschel did not live to examine fully the facts he supposed he had accumulated in and after 1817. He did not do much, indeed, towards carrying out the scheme which he had indicated. The papers of 1817 and 1818 may be compared respectively with those of 1784 and 1785; and I venture to say, that if in 1818 Herschel had had before him as long an observing career as in 1785, we should have found him definitely abandoning the principle he indicated in 1817, precisely as we find him in 1802 definitely abandoning the principle he indicated in 1784.

The fact is, both principles were matters to be tested, not to be adopted as rules for guidance.

William Struve adopted a singular combination of the two methods, and interpreted the results he obtained in a most remarkable, and, as I think, altogether unsatisfactory, manner.

Struve perceived that if the principle of star-gauging had been sound, the stars of the brighter orders ought to be strewn with general uniformity over the heavens; for the range of naked-eye vision, or low telescopic power, for separate stars lies far within the limits of the stellar system, even in its narrowest parts—that is, measured in the direction of its thickness. He saw then that here was a new test applicable to the star-gauging principle. It was only necessary to take some complete catalogue of stars down to, say, the 7th or 8th magnitude, and to try whether the stars grew richer as the Milky Way was approached. If they did grow richer, then it would be manifest that the principle of star-gauging could not be trusted *in all its generality.* (The reader will presently see why I emphasise these four last words.)

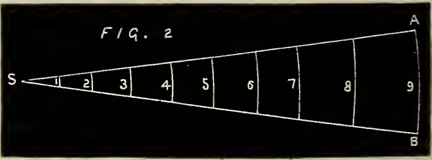
Struve took a catalogue complete only for an equatorial zone 30° wide, and extending 15° from the equator on the north and on the south. The catalogue was tolerably complete down to the ninth magnitude inclusive. Like other catalogues, it was numbered and arranged in order of Right Ascension; and what Struve did was simply to ascertain how many stars it included in the different 'hours.' He found that the Milky Way 'hours,' the seventh and nineteenth, were the richest, and that there was a gradual increase up to the maximum of richness, and then a gradual decrease down to the minimum ; so that there was all the evidence his method could afford to show that the nine brighter orders of stars do increase in numbers as the Milky Way is approached.

If he had been satisfied with this result, Struve would simply have obtained rough evidence of those laws of stellar distribution which my methods have more particularly indicated.

But he was not so satisfied, and it was in analysing his result that he applied the novel considerations I have spoken of.

He first assumed that the law of distribution for an equatorial zone 30° wide might be regarded as the law of

FIG. 2



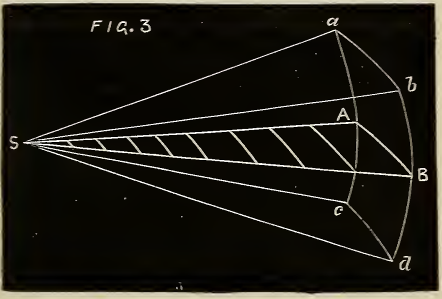
distribution for the circuit of the equator itself—a particularly daring assumption, when we remember that the galaxy does not cross the equator at right angles but slantwise, so that an 'hour' of stars 30° in declination range crosses parts of the star-sphere of very different richness (assuming, as Struve did, that the mid-zone of the galaxy is the zone of greatest richness). This first step, then—this substitution of the equator itself for an equatorial zone covering more than a quarter of the whole heavens—was directly calculated, it appears to me, to mask the real features of stellar distribution.

But the next step was even more daring. Having found that there is a variation of star-richness round the equator, two opposite points of which circle mark the place of maximum richness, Struve next changed the circle into a disc, and in this way :—

Suppose A B, fig. 2, to be one 'hour' of the celestial equator, S the place of the solar system. Then he took the stars which he had distributed uniformly along the arc A B, and spread them (according to the numerical distribution of their different orders of brightness) over the sectorial area A B S.

Surely an amazing application of the laws of averages, to take the stars spread over the area *a b c d* of the celestial sphere, where A B is an arc of 15° and *a b*, *c d* each of 30°, and to conceive these stars spread, according to a special law of uniformity, over the sectorial plane A B S (fig. 3). Yet it was thus that Struve's celebrated *section* was obtained.

FIG. 3



I hesitate to point out objections against this process because I cannot conceive what arguments can possibly be urged in its favour.

Struve himself considered that his result agreed perfectly with the views of Herschel in 1817 and 1818 ; and it will be perceived that there is something in the formation of the sectorial star-area S A B, which bears a distant resemblance to Herschel's resolution test of star-distance. On the other hand, Struve perceived that his results were altogether opposed to the general theory of star-gauging. He adopted, however, a modified form of the principle, since he assumed a certain very peculiar law of uniformity of distribution.

[In passing, I may remark that, in answering the first paper I read on this subject at the Royal Astronomical Society, Professor Pritchard made the somewhat remarkable statement that all Herschel's results confirmed each other, and that Struve's agreed with them !]

The plan on which I have proceeded has been unlike any of those hitherto described, and the principle I have adopted is not an assumption, but one whose justice depends on manifest and sufficient considerations.

I have, in the first place, set equal surface charting—as the only effective way of indicating laws of distribution, if any such exist—and charting *generally* as a necessary substitute for mere cataloguing, or other process of numerical estimation, whether as applied to the stars of various orders, to nebulæ, to clusters, to double, coloured, or variable stars, or, lastly, to star-motions. You can *see* a chart, study its relations, return again and again to its investigation, compare it with other charts, or combine it with them in a new chart, and so on. Any characteristics which charts are competent to exhibit, they show to all, and reveal independently of any special process. The case is altogether different with catalogues. Their capacity for concealing the truths really contained in them, and for mystifying the student of nature, and their adaptability for deceptive processes of manipulation, have been repeatedly illustrated in scientific researches.

But of course, when once a chart has been drawn, it suggests questions for statistical research ; or rather it suggests really effective methods of dealing with numerical relations. And the construction of charts of distribution has led me to notice two principles of analysis, which are manifestly sound, and have already been shown to be effective, while I believe very confidently that they will hereafter be found to be the only principles which can enable us to make any satisfactory advance in dealing with those portions of space which lie beyond our means of measurement.

They are these :—

A. If objects of one class are found to be spread *over certain parts of the celestial sphere* more richly than over others, and if objects of another class are found to exhibit corresponding peculiarities of distribution, being always more richly spread or more sparsely strewn where the same is observed of objects of the former class, then the two classes of objects form one system, and are intermixed within that system, the same *subordinate* region of *space* within that system including the aggregations of both classes of objects.

B. If, on the contrary, a law of contrast is observed in the distribution of objects of two different classes, so that the distribution of the objects of one class is *systematically* richer or poorer according as the distribution of objects of the other class is poorer or richer; then the two orders of objects form one system, but are separated from each other within that system, those subordinate regions of space which include many objects of one class having few or no objects of the other class, and *vice versâ*.

I will illustrate these principles before passing on to a third, which is only *probable*, not as these are *certain* (where only the evidence described can be obtained in sufficient *amount*).

Suppose the whole sky were covered more or less richly with flying creatures; suppose it could be seen that there were two well-marked classes of these creatures, large ones and small ones : then if, wherever the large ones were richly strewn over *the sky*, the small ones were likewise so strewn, while where there were few large ones there were few small ones, it would be a safe inference that the large and small ones were intermixed in the *aerial spaces*, travelling together in flights or groups. They might be old birds and their small youngsters, or birds pursuing their prey among flights of insects, or the like ; but whatever the two classes might be, it would be certain (supposing always that the peculiarity in question were sufficiently marked) that the large and small objects were intermixed.

But suppose the reverse held, and that where there were many large flying creatures, there were few or no small ones, and *vice versâ*, the peculiarity being very marked. Then it would be manifest that in this case also there was a law of association, but one which caused the objects of the two classes to separate into different groups. The large objects might be birds of prey, and the smaller might be weaker birds, banding together for mutual protection, or the like ; but it would be certain that the peculiar law of arrangement observed among the flying creatures was due to some real relation between the two classes, this relation causing them to keep apart from each other.

In neither case—and let the reader notice specially, that what I am about to say applies equally to both cases—in neither case could we imagine for a moment that the large flying creatures were of the same class as the small ones, but much nearer ; for it would be practically impossible that by some strange accident two strata of creatures, flying at different levels, should have their rich and poor regions so adjusted, as to be seen, in all cases, in the same direction, by the observer on earth.

This leads me to the third principle, which only affords probable evidence, though, in some cases, such evidence may approach very nearly to demonstration.

C. If objects of two classes are spread over certain parts of the heavens more richly than over others, but the rich regions of one agree systematically neither with the rich nor with the poor regions of the other, then the probability is, that the two orders of objects lie at different distances, and are in no way connected with each other.

This probability rises into certainty where some markedly peculiar arrangement of one order is seen to be carried across some equally marked arrangement of the other, the two arrangements being manifestly independent of each other.

The reader will perceive that the principles thus enunciated are those which I have followed in the interpretation of all my equal surface charts, whether of stars or nebulæ. Applied to my chart of 324,000 stars, principle A shows conclusively that neither the earlier nor later view of Sir W. Herschel respecting the Milky Way, nor the theory of the elder Struve, is even an approach to the true theory ; *but*, the *general* views indicated by Sir W. Herschel during the period of his observing career, intermediate between the abandonment of the star-gauging principle and the adoption of the resolution-test, accord fairly with the observed facts. The point overlooked by him, by Struve, and by all other astronomers, except the younger Herschel (who, however, did not sufficiently dwell on its importance), is the evidence of the intermixture of stars of many orders of real magnitude within the same regions of space. And as principle A, applied to the teachings of my chart of 324,000 stars, shows the Milky Way to consist of a system of star-streams, including many orders of stars, from the highest down to what must be regarded relatively as mere star-dust, so principle B, applied to my charts of Nebulæ (and still more clearly when applied to the exact charts recently submitted by Mr. Sidney Walters to the Astronomical Society), shows that the star-cloudlets belong to the same system as the stars of the Galaxy, but are so distributed within that system as to be rich where stars are relatively few, and few where stars are relatively rich.

But much wider and more systematic research is still required. It will be manifest that the three principles, but especially the first, admit of being applied most effectively to stellar research—much more effectively than star-gauging as originally proposed, even if it had turned out that the principle of such star-gauging was sound. By applying telescopes of different power to systematic star-gauging, as distinguished from the mere selection of a few widely scattered fields, and by employing for each survey only dark, clear, and moonless nights, instead of using (as was the case with Herschel's surveys) moonlight, twilight, and hazy nights, and even daylight, an estimate can be formed of the laws of distribution of the stars of various orders, proceeding onwards from those included in my chart of 324,000 stars, and thus extending and amplifying the teaching commenced in that chart. When that work has been accomplished, we shall begin to understand the real wonders of the star-depths, the magnificence of the subordinate star-schemes which have been mistaken for the sidereal system itself, and something of the grandeur of that system, whose limits lie far beyond the range of our most powerful telescopes, while within them are included all the various orders of celestial objects which the telescope has yet revealed. Combining such lessons with what has been learned and yet remains to be learned of the movements taking place within the sidereal universe, we shall have a picture grander and more impressive by far than any yet presented to our contemplation; we shall learn that the true Galaxy is infinitely more extended, infinitely more complex in structure, than we have supposed, and that the processes at work within its bounds are infinitely more stupendous.

TRANSITS OF VENUS.

DURING the autumn months of 1882 the evening star was seen drawing nearer and nearer, night after night, to the place of the sun, until at length she set too soon after him to be discernible save with the telescope. To the astronomer this approach of Venus to the sun had an interest greater than usually attaches to the phenomenon; for it was known that she would not pass from the eastern to the western side of the sun's orb without crossing his face. The passage of Venus close by the sun is, of course, a phenomenon of frequent occurrence and possessing no special interest. Hesperus, the star of evening, cannot change into the morning star, Lucifer, without passing the sun's place upon the heavens; nor can Lucifer change into Hesperus without a similar passage; though there is a distinction between the two cases, for it is by passing between the sun and the earth that the evening star changes into the morning star, while it is by passing beyond the sun, so that the sun comes between the earth and her, that Venus changes from a morning star into an evening star. But these are astronomical phenomena which have been witnessed and understood for thousands of years. It is when Venus, in passing from the east to the west of the sun, does not steer clear of his disc, but traverses it, so that she appears in the telescope like a round black spot upon his face, that every astronomer is interested. For these occasions are few and far between. The last transit of Venus before 1874 occurred more than 105 years earlier; and although that transit was followed by another in 1882, yet after that second transit an even longer interval will elapse before another occurs, than has passed since the transit of 1769 : not until June 2004 will Venus again pass over the face of the sun. And besides the interest naturally attaching to a phenomenon which occurs so seldom, the transits of Venus have a scientific importance depending on their relation to celestial measurement, since they afford the best means astronomy possesses for determining the distance of the sun, and with that distance the dimensions of the solar system, besides whatever information we may hope to possess respecting the tremendous distances which separate the sun from his fellow-suns, the stars.

Transits of the inferior planets were, at first, only awaited with interest because of their bearing on the Copernican theory of the solar system. It was not until astronomers had abandoned the old systems that they could have any positive assurance that any planet ever passed between the earth and the sun. Moreover, regarding the celestial bodies as all self-luminous, astronomers could hardly have expected that, even if a planet passed across the sun's face, it would be discernible as a dark spot. We do, indeed, hear that some old observations of sun-spots were regarded as transits of Mercury or Venus across the sun's disc. Thus, the author of the 'Life of Charlemagne' tells us that Mercury was seen in April 807, as a black spot upon the sun's face, for eight consecutive days. Kepler, who was perfectly well aware that Mercury could not remain as many hours on the sun's disc, endeavoured to show that the expression used in the manuscript of the old writer might not have been *octo dies*, but *octoties*, a barbaric form of *octies* for eight times. Again, the famous physician Ebn Roschd (commonly called Averroës) says, in his Ptolemaic Paraphrase, that in the year 1161 he saw Mercury on the sun at a time when the planet really was in inferior conjunction (that is, passing between the earth and the sun). Kepler himself believed that he had so seen the planet. In his day Mercury was supposed to be a much larger body than it actually is. Hence there was nothing surprising in the fact that an experienced astronomer like Kepler should have mistaken for the planet a sunspot, seen no doubt only for a short time when the sun was low down. But later, when the telescope had revealed the existence of spots upon the sun's face, Kepler admitted that in all probability he had seen such a spot, and not Mercury. We know now that even Venus, much larger though she is than Mercury, and much nearer to the earth when in transit, is quite invisible to the unaided eye at such a time.

Gassendi, who was the first to witness a transit of an inferior planet, saw Mercury pass across the face of the sun on November 7, 1631. His account of the observation is quoted somewhat fully in the *Cornhill Magazine* for November 1868. Kepler, who had announced the transit, had also predicted a transit of Venus on December 6, 1631; and Gassendi hoped to witness this event. Kepler predicted that the transit would begin shortly before sunset; but as the transit of Mercury had not occurred exactly at the time indicated by Kepler, Gassendi thought that quite possibly he might witness the whole of the transit of Venus. He was prevented from observing the sun on December 4 and 5 by impetuous storms of wind and rain. 'On the 6th he continued to obtain occasional glimpses of the sun, till a little past three o'clock in the afternoon, but no indication of the planet could be discerned.' 'On the 7th he saw the sun during the whole forenoon, but looked in vain for any trace of the planet.' We now know that the transit took place during the night between December 6 and December 7.[1]

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[1] It is commonly stated that no part of the transit could have been witnessed in Europe. The present writer, however, having calculated the circumstances of the transit as accurately as the case warrants, finds that the end of the transit could have been seen from the south-eastern parts of Europe, occurring at sunrise for all places on a line drawn from Gibraltar through Marseilles, Dresden, St. Petersburg, to the extreme north-east of European Russia. The account of Gassendi's failure in the excellent treatise *Les Passages de Vénus*, by M. Dubois, Naval Examiner in Hydrography for France, is amusing: 'Le passage de Vénus,' he says, 'qui sans doute n'était pas prédit avec une pré-

Just as the transit of December 1874 was followed by another December transit in the year 1882, so the transit of December 1631 was followed by another in December 1639. The earlier had escaped observation, as we have seen, though predicted by Kepler; the latter, which according to Kepler's tables would not take place, was observed, though it almost escaped the ingenious astronomer who detected the mistakes in Kepler's computations and watched for its occurrence. This astronomer was one whose name is only not associated with any great discoveries because he died so young. Had he lived it is probable that Newton himself would not have stood much higher among the astronomers of England. Jeremiah Horrocks, minister of Hoole, in Lancashire (aged only twenty), had in his zeal for science gone over the computations published by Kepler in the Rudolphine Tables. Comparing these with Lansberg's Tables of the Motions of Venus, he noticed that, while according to Kepler the planet would pass very close to the sun, but south of his disc, on December 4, 1639, Lansberg's Tables assigned to the planet at that conjunction a course traversing the northern part of the sun's disc. He had found Kepler a much more reliable authority than Lansberg; but he had reason, from his own observations, to believe that Venus would follow a course between the two paths thus assigned by Lansberg and Kepler—somewhat nearer to Kepler's—insomuch that, instead of passing south of the sun, she would transit the southern part of his disc. He determined, therefore, to watch carefully for this interesting phenomenon. 'Lest a vain exultation should deceive me,' he says, 'and to prevent the chance of disappointment, I not only determined diligently to watch the

cision suffisante, ne fut pas observé, *d'abord* parce que Gassendi, qui s'apprêtait à l'observation, en fut empêché par la pluie, mais *surtout* parce que le passage eut lieu pendant la nuit pour les observateurs Européens.' It may reasonably be admitted that the occurrence of the transit when the sun was below the horizon was a sufficient cause for Gassendi's failure, apart from the rain.

important spectacle myself, but exhorted others whom I knew to be fond of astronomy to follow my example, in order that the testimony of several persons, if it should so happen, might the more effectually promote the attainment of truth, and because by observing in different places our purpose would be less likely to be defeated by the accidental interposition of clouds or any fortuitous impediment.' In fact, he was not free from astrological fears pointing to such interposition, since the positions of the planets Jupiter and Mercury seemed to portend bad weather. 'For,' he remarks, 'in such apprehension I coincide with the opinion of the astrologers, because it is confirmed by experience; but, in other respects, I cannot help despising their puerile vanities.'

Horrocks's description of his successful observation is interesting in many respects, especially perhaps for the enthusiasm which pervades it. 'On voit,' says Delambre in his 'History of Modern Astronomy,' 'que Horrocks était jeune et enthousiaste, mais cette jeunesse et cet enthousiasme annoncaient un homme vraiment distingué.' 'Following the example of Gassendi,' Horrocks begins, 'I have drawn up an account of this extraordinary sight, trusting that it will not prove less pleasing to astronomers to contemplate Venus than Mercury, though she be wrapt in the close embraces of the sun—

Vinclisque nova ratione paratis
Admisisse Deos.

Hail! then, ye eyes that penetrate the inmost recesses of the heavens, and gazing upon the bosom of the sun with your sight-assisting tube, have dared to point out the spots on that eternal luminary! And thou, too, illustrious Gassendi, above all others, hail! thou who, first and only, didst behold Hermes' changeful orb in hidden congress with the sun. Well hast thou restored the fallen credit of our ancestors, and triumphed o'er the inconstant Wanderer. Behold thyself, thrice celebrated man! associated with me, if I may venture so to speak, in a like good fortune. Contemplate, I repeat, this most extraordinary phenomenon, never in our time to be seen again! the planet Venus, drawn from her seclusion, modestly delineating on the sun, without disguise, her real magnitude, whilst her disc, at other times so lovely, is here obscured in melancholy gloom; in short, constrained to reveal to us those important truths which Mercury on a former occasion confided to thee. How admirably are the destinies appointed! How wisely have the decrees of Providence ordered the several purposes of their creation! Thou! a profound divine, hast honoured the patron of wisdom and learning; whilst I, whose youthful days are scarce complete, have chosen for my theme the Queen of Love, veiled by the shade of Phœbus' light.'

Horrocks was fortunate in possessing a telescope of considerable power (for that period). He says that it showed even the smallest spots upon the sun, and enabled him to make the most accurate division of the solar disc. Moreover, he was already, notwithstanding his youth and the recentness of the invention, familiar with the use of the telescope, and he remarks respecting the instrument he employed that in all his observations he had found it represent objects with the greatest truth.

Horrocks's calculations must have been made with great care and excellent judgment. We have seen that Gassendi's attempt to observe the transit of 1631 had failed, because Kepler's computations had been so far erroneous that, instead of the transit beginning before sunset on December 6, it really began nearly at midnight (for Paris or Greenwich) between December 6 and December 7. Halley's computation of the transit of 1761 was also seriously in error (about half an hour); and an error of fully an hour was made in the first published statement respecting the transit of the year 1874! We shall see that Horrocks was correct within a few minutes. He had found that other astronomers set the conjunction of 1639 as occurring on November 23 (old style), whereas his own calculations 'forbad him to expect anything before three o'clock in the afternoon of the 24th' (December 4, new style.) Fearing, however, lest 'by too much self-confidence' he might 'endanger the observation,' he watched the sun through the greater part of the 23rd, and the whole of the 24th. 'I watched carefully,' he says, 'on the 24th from sunrise to nine o'clock, and from a little before ten until noon, and at one in the afternoon,—being called away in the intervals by business of the highest importance, which, for these ornamental pursuits, I could not with propriety neglect.' As the day was Sunday, we may gather from this that the church services in small places like Hoole, in the seventeenth century, lasted not much longer than the low mass of a century earlier; but that a longer service took place at one, unless (as seems not unreasonable) we are to suppose that Horrocks took his dinner between one and three, soon after which hour he resumed his observation of the sun. 'About fifteen minutes past three,' he says, 'when I was again at liberty to continue my labours, the clouds, as if by Divine interposition, were entirely dispersed, and I was once more invited to the grateful task of repeating my observations. I then beheld a most agreeable spectacle—the object of my sanguine wishes—a spot of unusual magnitude, and of a perfectly circular shape, which had already fully entered upon the sun's disc on the left, so that the limbs of the sun and Venus perfectly coincided. Not doubting that this was really the shadow of the planet, I immediately applied myself sedulously to observe it.'

We need not consider very closely what Horrocks actually observed; for of course no special scientific interest attaches to the details of his observations. The chief point is that his prediction should have been so closely fulfilled. The time, indeed, during which he could examine the appearance and motions of Venus was very short. He had first seen her at a quarter past three, and the sun set thirty-five minutes later. Nevertheless, he effected one discovery worthy of notice. He found that the planet's apparent size as very much smaller than had been supposed. Gassendi had effected a similar discovery respecting Mercury. Thus had the transits of these two planets shown that relatively to the sun their globes are much smaller than astronomers had imagined.

Horrocks had written to his friend Crabtree, a young man well skilled in mathematics and astronomy, 'inviting him to be present at this Uranian banquet, if the weather permitted.' 'But the sky,' says Horrocks, 'was very unfavourable, being obscured during the greater part of the day with thick clouds; and as he was unable to obtain a view of the sun, he despaired of making an observation, and resolved to take no further trouble in the matter. But a little before sunset—namely, about thirty-five minutes past three—the sun bursting forth from behind the clouds, he at once began to observe, and was gratified by beholding the pleasing spectacle of Venus upon the sun's disc. Rapt in contemplation he stood for some time motionless, scarcely trusting his own senses, through excess of joy; for we astronomers have, as it were, a womanish disposition, and are overjoyed with trifles, and such small matters as scarcely make an impression upon others; a susceptibility which those who will may deride with impunity, even in my own presence; and if it gratify them I too will join in the merriment. One thing I request: let no severe Cato be seriously offended with our follies; for, to speak poetically, what young man on earth would not, like ourselves, fondly admire Venus in conjunction with the sun, *pulchritudinem divitiis conjunctam?*'[1]

None but these two saw the transit of 1639. It might have been observed under more advantageous conditions by astronomers in Spain. In France it could have been seen nearly as favourably as in England ; but in the eastern parts of Europe it could not have been seen at all. It would have been favourably seen from the greater part of the North American Continent, had there been any astronomers there to study it.

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[1] This passage, like the others quoted respecting the transit of 1639, is from the translation of Horrocks's original memoir, by the Rev. Arundell B. Whatton. The sketch of Horrocks's life, accompanying Mr. Whatton's translation, is full of interest. We are glad to see that the Astronomical Society has honoured itself recently by having a tablet placed in Westminster Abbey in memory of Horrocks.

Many years passed before the astronomical world began again to consider the subject of a transit of Venus. It was not, indeed, until June 1761 that another was to take place. Towards the close, however, of the seventeenth century, the astronomer Halley, who during his stay (when very young) at St. Helena had observed a transit of Mercury, published an interesting dissertation showing how a transit of Venus might be so observed as to afford means for determining the distance of the sun. In this paper he discussed the circumstances of the transit of 1761, according to his calculation respecting the time and manner of its occurrence. It was then that he described the method of observing a transit, which is commonly called Halley's, though it may also be called (if we do not wish to give Halley his due) the method of durations. Probably the simplest sketch of this method would be thought out of place in these pages. We shall therefore content ourselves with merely noting that it depends on observing the duration of the transit as seen at different stations. If our earth were a mere point compared with the sun, the circumstances of a transit would be appreciably the same from whatever part of the earth it was observed. But as the earth has dimensions which, though small, are yet measurable compared with the sun's, observers in different parts of the earth see a transit under different circumstances; and amongst other circumstances affected in this way, the duration of the transit may be longer or shorter according to the observer's position. It does not matter whether the difference be brought about by setting one observer far to the north and another far to the south, or by taking advantage of the rotation of the earth which in a transit of long continuance shifts the place of an observer who is near the equator very importantly, while scarcely at all affecting an observer placed in a high latitude, or even shifting him in a contrary direction if he is on that side of the arctic regions which lies farthest from the sun. It is manifest that the larger the earth compared with the sun's distance, the greater will be the effect due to difference of position ; in other words, the difference of duration will be greater. And nothing can be simpler than the measurement of the transit's duration as observed at any place. All that is necessary is a clock which will not gain or lose appreciably during the time that the transit is in progress. Having determined the difference for stations of known position on the earth, we are enabled to infer what proportion the earth's dimensions bear to the sun's distance, or, in other words, we learn how far off the sun is.

Halley, then, in the dissertation to which we refer, proposed that, during the transit of 1761 observers should be placed at certain stations which he pointed out—at Bencoolen in Sumatra, at an arctic station near Hudson's Bay, and so on—where the transit would have its greatest and its least duration, so that by finding how great the difference of duration might be, the observers would be enabled to infer the relation which the earth's dimensions bear to the distance of the sun.

The remarks with which Halley closed the introductory portion of his dissertation are worth quoting for the fine scientific spirit which pervades them:—'I could wish,' he says, 'that many observations of this famous phenomenon might be taken by different persons at separate places, both that we might arrive at a greater degree of certainty by their agreement, and also lest any single observer should be deprived, by the intervention of clouds, of a sight which I know not whether any man living in this or the next age will ever see again, and on which depends the certain and adequate solution of a problem the most noble, and at any other times not to be attained to. I recommend it therefore again and again to those curious astronomers who, when I am dead, will have an opportunity of observing these things, that they would remember this my admonition, and diligently apply themselves with all their might in making this observation ; and I earnestly wish them all imaginable success ; in the first place that they may not, by the unseasonable obscurity of a cloudy sky, be deprived of this most desirable sight, and then, that having ascertained with more exactness the magnitudes of the planetary orbits, it may redound to their immortal fame and glory.'

As the transit of 1761 drew near, careful observations of the motion of Venus were made, which showed that she would not transit the sun in the manner predicted by Halley. It was also found that no stations could be reached at which Halley's method could be conveniently applied. In fact the transit was not one whose whole duration could be advantageously observed. The French astronomer Delisle proposed another method, more difficult in an astronomical sense, but geographically much more convenient. We have already said that, owing to the fact that the earth has dimensions measurably comparable even with those of the solar system, the circumstances of a transit will be different at different stations. Amongst other circumstances so affected will be the time at which transit will begin or end. Halley's method was directed to the observation of the time transit *lasts*, and the differences observed in such duration were to give the means of determining the sun's distance. Delisle suggested that observers might note the time at which transit *began or ended* (not necessarily observing the whole transit), and that differences observed in the epochs noted would give the means of measuring the sun's distance. Of course all the observed epochs would have to be expressed according to a uniform manner (all in Greenwich time, for instance, or all in Paris time) ; so that the longitude of each observing station would have to be determined as well as the local time at which transit began or ended. But this accomplished, the method would avail as well as Halley's for determining the sun's distance.

Accordingly, the French Academy made preparations for sending out observers to stations suitable for applying this method, which (if we wish to avoid naming Delisle), may be called the 'absolute time method,' because it depends on comparing the absolute instants at which transit begins or ends at different stations. Le Gentil, most unfortunate of men so far as transits of Venus were concerned, was sent to Pondicherry, but, as we shall presently see, did not arrive there. Here the whole duration could be observed, but his special object was to observe ingress. Chappe d'Auteroche was sent to Tobolsk in Siberia. England, with fine official pertinacity, held fast to Halley's selection, Bencoolen, though in the actual circumstances of the transit Bencoolen presented no advantages whatever. Doubtless an expedition would have been sent to Halley's other station near Hudson's Bay, but for the circumstance that the transit would not have been visible there at all.

The actual history of the expeditions is full of interest, but would require more space than can here be devoted to it. Fortunately for science, the ship which had set out for Bencoolen was attacked by a Spanish vessel of greatly superior strength, and being compelled to put in at the Cape of Good Hope, an excellent station for observing egress in Delisle's manner, observations were made which proved far more serviceable than any which could possibly have been made at Bencoolen. Le Gentil set forth for Pondicherry on March 26, 1760. Had he reached that station, he would have witnessed the curious spectacle of a transit, the middle of which occurred with the sun almost vertically overhead. But when Le Gentil arrived at the Isle of France (on July 10, 1760), he learned that a war had broken out between France and England, and that it would be unsafe for his ship to proceed. He had resolved to betake himself to Rodriguez (which would, however, have been a most unfortunate selection, as only the egress would have been visible, and under very unfavourable conditions), when he learned that a French frigate was about to sail for the coast of Coromandel. In her, therefore, he determined to proceed to

Pondicherry. He sailed from the Isle of France in the middle of March 1761, and after experiencing many provoking delays from calms, reached Malabar on May 24, only to learn that the English were masters of Pondicherry. The captain of the frigate sailed away with all speed for the Isle of France, and she was still on her way thither when the day of the transit arrived. Le Gentil made most ingenious preparations to observe the transit, and favoured by splendid weather he had an excellent view of its phenomena. But it need hardly be said that the observations he made, however interesting to him as a student of astronomy, were utterly valueless for the determination of the sun's distance. As Dubois well remarks, 'Le Gentil had experienced one of those mishaps which assume to the man of science all the proportions of a real misfortune ; to have traversed so large a portion of the globe, to have endured all the weariness, all the privations, all the perils, of a long sea-voyage, and to effect nothing, this was enough to have disgusted anyone with scientific observation, or, at least, with Halley's method.' But, as we shall soon see, Le Gentil was even now not at the end of his troubles. Chappe d'Auteroche, after a long and painful journey, reached Tobolsk, and observed the transit there under favourable conditions.

The results of the observations of the transit of 1761 were by no means so satisfactory as had been expected, so far as the determination of the sun's distance was concerned. Some estimates made the distance nearly 100 millions of miles, while, according to others, the sun's distance fell short of 80 millions of miles. Astronomers had already obtained much more satisfactory measurements from observations made upon the planet Mars; so that the most striking result of the transit observations made in 1761, seemed to be the recognition of the inferiority of such observations compared with other methods available to astronomers for determining the sun's distance.

It was, in fact, during the transit observations of 1761 that astronomers recognised a peculiarity in the behaviour of Venus as she enters upon and leaves the sun's disc, which militates very strongly against the usefulness of both Halley's and Delisle's method. Theoretically nothing can be more perfect than the plan suggested for timing durations by one method, and the absolute moment of ingress or egress by the other. As the round black disc of Venus passes upon the sun's face at ingress, the observer can wait for the moment when her outline will just touch the sun's. As this moment gradually approaches, he can give his whole attention to determine the exact instant when the two outlines are in contact; and theoretically this instant can be exactly determined: for, the moment *after*, the sun's light will be seen between the two outlines. But practically matters do not proceed so conveniently. In the first place, the outline of orb is rippled through the effects of atmospheric undulations, and so much the more disturbed as the sun is nearer the horizon,—a fact of great importance, because all the best stations have the sun somewhat low down at the critical moment of contact. Then there is the optical effect called irradiation, by which the image of a bright object on the retina is apparently enlarged. This effect, of course, makes the dark disc of Venus look smaller, and the bright disc of the sun look larger than they really are; and a very little consideration will show that at the moment when the two are really in contact (Venus just lying wholly within the sun's disc) instead of appearing as a round black disc just touching the sun, she will appear to have a disc smaller than her real orb, and not round, since irradiation will not contract the disc at the place of contact as it does elsewhere. Thus at the moment of real contact Venus will present a pear-shaped aspect, the stalk of the pear connecting the body with the edge of the sun's disc. The moment after real contact, the stalk will appear to break, and either in an instant or very quickly the pear-shape will disappear, and the disc will become circular, with a wide space separating it from the edge of the sun's disc. The disc will seem to have taken a leap, as it were, from the sun's edge, to which a moment before it was attached by the black stalk. Similar phenomena will present themselves at egress in a reverse order. Now, these effects are combined with those due to atmospheric undulations; and moreover the extent of irradiation depends largely on the observer himself (some persons being much more sensitive to the effects of light than others), and largely also on the telescope employed, the state of the air, and other variable circumstances. So that manifestly, instead of that neat and precise determination of the moment of contact which Halley expected, and which both his method and Delisle's require, there must be considerable uncertainty in the comparison of observations made by different astronomers, at different stations, with different telescopes, and under different conditions.

Most of those who observed the transit of 1761 mention the occurrence of peculiarities such as we have here described. Thus, Mr. Hirst, who observed the transit at Madras, where the sun was at a considerable elevation, states that 'at the total immersion, the planet, instead of appearing truly circular, resembled more the form of a Bergamot pear, or, as Governor Pigott then expressed it, 'looked like a nine-pin,' yet the part of the disc farthest from the sun was extremely well-defined. When the planet was about to leave the sun similar appearances were presented. 'The planet was as black as ink, and the body truly circular just before the beginning of egress, yet it was no sooner in contact' with the edge of the sun's disc 'than it assumed the same figure as before,' the other part of Venus 'keeping well-defined and truly circular.'

However, the general impression among the astronomers of the last century would seem to have been that too much reliance had been placed on Delisle's method. Cassini, several years later, wrote as follows respecting the arrangements for the transit of 1769 :—'Experience is our chief instructor; the fruit of her lessons repays us for the years passed in learning them. In 1761 the principal object had been missed for want of observations in places where the durations differed sufficiently. It was essential not to experience a second time the same disadvantage.'

In accordance with this view, preparations were made for sending observers in 1769 to the South Sea, California, Mexico, Lapland, Kamschatka, Hudson's Bay, and other places where the whole transit could be observed. England took an important part in these arrangements. At that period Spain possessed nominal dominion over the South Sea, and the French Government waited for permission from Spain to observe the transit in those seas. This permission Spain refused to grant. But England did not wait for it. Indeed, from all the accounts we have seen, it does not appear that the idea of asking Spain for permission to visit the Southern Seas occurred to the British authorities. The Royal Society presented a memorial to George III. early in 1768, requesting, among other things, that a vessel might be fitted out at the expense of Government 'to convey proper persons to observe the transit, either from the Marquesas, or from one of those islands to which Tasman had given the several appellations of Amsterdam, Rotterdam, and Middleburgh," now known as the Friendly Isles. This petition was readily complied with. But early in the negotiations a hitch occurred which threatened mischievous delays. Dalrymple, who had been selected to superintend the expedition, was a man eminent in science, and every way worthy of confidence; moreover he had 'already greatly distinguished himself respecting the geography of the Southern Ocean. As this gentleman had been regularly bred to the sea, he insisted on having a brevet commission, as captain of the vessel, before he would undertake the employment.' The Admiralty violently opposed this measure, and Lord Hawke (who then presided at the Admiralty) 'declared that nothing could induce him to sanction such a commission.' After much debate, both sides proving inflexible, it was thought desirable to look out for another commander for the expedition, and Captain Cook (then Lieutenant and afterwards world-renowned) was eventually appointed. The

'Endeavour,' a barque of 370 tons, originally built for the coal trade, was selected as a suitable vessel, and at the suggestion of Captain Wallis, who had just returned from a voyage round the world, Otaheite (then called King George's Island) was chosen as the most convenient place for observing the transit. The observers selected were Mr. Charles Green, assistant of Dr. Bradley, the Astronomer Royal, and Mr. Joseph Banks (afterwards the President of the Royal Society). Two draughtsmen, a secretary, and four subordinate assistants accompanied the observers. Solander, the Swedish naturalist, also sailed with Cook, and his botanical observations were among the most important fruits of the expedition. The transit was successfully observed both by Green and Banks.

Le Gentil experienced in 1769 the culmination of his misfortunes as a transit observer. With a pertinacious courage worth of better success, he determined, after his failure in 1761, to return to Pondicherry so soon as an opportunity presented itself, and there to await the transit of 1769. For eight years he waited, employing himself in the agreeable study of Brahminical astronomy. But, alas! when June 3, 1769, arrived, an envious cloud covered the sun at the moment when Le Gentil was preparing to reap the reward of his patience; and all that was left to the unhappy astronomer was to return to France and publish a book on the astronomy of the Brahmins. Let us hope that he was kindly treated by the critics.

As important as observations in the South Seas, where the duration of the transit was shortened, were those made at Wardhuus, in Lapland, where the duration was lengthened. The King of Denmark invited Father Hell, a skilful German astronomer, to occupy this station in company with the Danish astronomer Borgrewing. Arriving at Wardhuus in the autumn of 1768, the two astronomers wintered in that desolate region; and fortunately, when the day of the transit arrived, clear weather permitted them to make good observations. Doubt has, indeed, been thrown upon the observations of Hell, in comparatively recent times, because of the difficulty of reconciling them with the present estimates of the sun's distance. The Astronomer Royal has even gone so far as to suggest that the worthy Father was asleep at the moment of egress, and that being ashamed to admit the fact he made an entry in his notebook describing an imaginary observation. We know of nothing rendering this at all probable, for Hell was a man held in high esteem by his contemporaries. The time, indeed, at which egress occurred was such that sleep would not in itself have been an improper indulgence. Transit began, at Wardhuus, at about half-past nine in the evening, and ended at about half-past three in the morning (there was no night in that high latitude), and a rest in the interval would have been excusable. But it seems absurd to suppose that the astronomer would have left his waking to chance. Besides, there were the astronomers Sajnowiz and Borgrewing, as well as several assistant observers, and these would not have left Father Hell to sleep through the important moments of egress. It seems likely, in fact, that his unlucky name, rather than any other circumstance, suggested a charge which, if really warranted, would expose him to the undying obloquy of astronomers. In this respect he may be compared to that unfortunate Dr. Impey, whom Macaulay represents to us in so contemptible an aspect,—with no better justification, according to well-informed historians.

A great deal has been said during the last few years about the error which astronomers are supposed to have detected in that estimate of the sun's distance which had been based on the observations of 1769. But in point of fact it was very early seen that these observations were little more trustworthy than those made in 1761. Within less than two years, upwards of *two hundred* papers containing different estimates of the sun's distance were sent by various persons to the Academy of Paris, and probably about four hundred to the different learned societies of Europe.

Selecting from among these the papers contributed by the able mathematicians, Lalande, Euler, Pingré, Hornsby, and Hell, we find the estimates of the sun's distance ranging from 92 millions to 96 millions of miles. Not the least curious part of the matter is that all the calculators were positive they were right. Pingré, who made the distance 92 millions of miles said, 'Of two things one, either this is the true distance, or the observations of 1769 are not to be trusted at all;' while Lalande said that 'incontestably' the sun's distance amounts to fully 96 millions of miles. Euler, a greater mathematician than either, after carefully going over his work afresh, obtained a value almost exactly midway between Lalande's and Pingré's. Then did Dionis du Séjour publish a new investigation leading to nearly the same value which Pingré had obtained. Lastly came Encke, who with German patience combined all the observations together, with a result nearly coinciding with Lalande's. This was the value of the sun's distance—95,265,000—which for so many years reigned in our books of astronomy; though why implicit reliance was given to it when the history of the investigation showed that mathematicians more skilful than Encke had obtained results differing widely from his, cannot be easily explained. Nor should it have been thought at all a wonderful circumstance that researches by other methods soon began to point to a different value of the sun's distance.

This would not be the place to explain the various methods by which, without the aid of a transit of Venus, astronomers have in recent times obtained new estimates of the scale on which the solar system is constructed. Yet the ingenuity with which the great problem has been attacked is so remarkable that it may interest our readers to have simply stated, without explanation of details, the contrivances employed by astronomers. First, there was an old method depending on the observation of the planet Mars, when at his nearest to us, and when therefore his apparent position in the heavens is most affected by the difference in the position of the observers on our earth. This method was applied in two ways: in one, by stationing observers far apart; in the other, by taking advantage of the fact that an observer is carried round by the daily rotation of the earth so as to have his place changed for him, so to speak. Then the motions of the moon were consulted. Our satellite is disturbed by the sun, and if her path were indefinitely small compared with his distance she would be just as much disturbed in the half of her path farthest from him, as in the half nearest to him; but as the sun's distance is not immeasurably superior to the moon's, she is slightly more disturbed when traversing the half of her path nearest to him than when traversing the other half. The excess of disturbance being noted, affords a means of estimating the sun's distance; for, as we have seen, it depends on the extent to which that distance exceeds the readily measured distance of the moon.[1] Then there was yet another method, depending on the fact that the earth circuits once a month around the common centre of gravity of her orb and the moon's, so that she is now a little on this side, now a little on that side, of the place she would have if there were no moon. This slight range on her part from what may be called her mean position gives, as it were, a base of measurement, to either extremity of which the astronomer is carried successively month after month, and the resulting slight displacement of the sun's apparent place shows itself in the records of Greenwich, Washington, Paris, and other great observatories. The sun's distance is inferred from such observations, just as a surveyor infers the distance of some inaccessible spire or rock by noting how much it is shifted in direction as seen from one or the other end of a measured base-line. Then there was that most ingenious and wonderful of all methods which depends on the measurement of the velocity of light. Everyone knows that astronomy first revealed the fact that light travels with a measurable though inconceivable velocity. The little satellites of Jupiter were found to undergo eclipse earlier or later according as Jupiter was nearer to or farther from us, and it was soon seen that these effects arise from the fact that the light-message by which the news of these eclipses is conveyed takes a longer time to traverse the longer distance,—in other words, that light does not travel with infinite velocity. It appeared from the observed effects that light occupied about seventeen minutes in traversing a distance equal to the diameter of the earth's orbit; and using Encke's value of the sun's distances, this implied that light travels with a velocity of about 192,000 miles per second. Of course if the sun's distance is greater, light travels more quickly, and if less then less quickly. It occurred to Foucault to apply an ingenious contrivance, devised by Wheatstone for measuring the duration of the electric spark, to the less difficult task of measuring the velocity of light. And inconceivable though it may seem that a velocity of nearly two hundred thousand miles per second can be measured by any terrestrial contrivance, the task was accomplished so satisfactorily that the resulting estimate of the velocity of light has been thought a sufficient ground for adopting a new estimate of the sun's distance. Foucault found that light does not travel at so great a rate as 192,000 miles per second, but at the rate of about 180,000 miles, so that the diameter of the sun's orbit must be less than Encke had supposed,—the sun's distance being reduced in this way from 95,265,000 miles to about 92 millions. The values obtained by the other methods all lie much nearer to this value than to Encke's, ranging in fact from 91,230,000 miles to 92,680,000 miles. So that whether Pingré and Dionis du Séjour were right or wrong in asserting that the transit observations in 1769 point to a solar distance of 92,000,000 miles, it is certain that modern observations point to such a distance.

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[1] The moon's distance is easily measured because she is so near that two observers at distant stations on the earth see her in perceptibly different directions.

Much has been said respecting the efforts which have been recently made to show that the observations of 1769 can be forced into agreement with the new and reduced estimates of the sun's distance. The continental astronomer Powalky effected this by selecting certain observations and rejecting others—without giving any sufficient reasons for so doing. Stone, of Greenwich, adopted a plan little more satisfactory, though many writers (the present writer among the number) somewhat hastily assumed that he had removed the whole difficulty. We have described the peculiarity which affects the appearance of Venus when she is just wholly upon the disc of the sun. Between the moment at ingress when her rounded outline seems to belong to a circle which (if complete) would touch the sun's outline (the moment of apparent contact) and the moment when she seems suddenly to break away from the edge of the sun (the moment of real contact), an interval elapses ; and there is a corresponding interval between the two contacts at egress. Mr. Stone found that if this interval be taken as seventeen seconds then the observations of 1769 point to just such a distance of the sun as astronomers have recently been led to adopt. This is all very well ; but if it proved anything it would prove that the interval either always amounts to seventeen seconds or that seventeen seconds is a fair average value. Even if this were true nothing else would have been demonstrated by Mr. Stone's investigation. But unfortunately those observers who, availing themselves of the experience obtained in 1761, were careful to observe both kinds of contact in 1769, found the interval to be not only widely variable but always much greater than seventeen seconds. Green at Otaheite found the interval to be 40 seconds at ingress and 48 seconds at egress. Cook made it 60 seconds at ingress and 32 seconds at egress. Maskelyne, the Astronomer Royal, observed a difference of 52 seconds, while Horsley, at the same station (Greenwich Observatory), found it to be 63 seconds. Hornsby at Oxford found the difference to be 57 seconds, while Shuckberg, also at

Oxford, found it to be fully 69 seconds. Yet all these observers were prepared for this peculiarity, and Maskelyne had issued special instructions for their guidance in this particular respect.[1] We cannot wonder, therefore, if continental and American astronomers unanimously decline to recognise any independent value in Mr. Stone's attempted reconciliation between the transit observations of 1769 and recent measurements of the sun's distance.

It was hoped, however, that the observations which were to be made during the transits of 1874 and 1882 would remove all doubt as to the correctness of these more recent measurements. At a very early date attention was directed to the transits by the Astronomer Royal for England ; who, in May 1857, delivered an address to the Astronomical Society, in which he described the various methods available for determining the sun's distance, and pointed out the advantages of a transit of Venus, more especially if it could be observed by Halley's method. He stated, however, that the method of durations could only be applied in 1882, any observable difference in 1874 'being probably little more than half as great as in 1882.' It appeared also from his calculations that to apply the method successfully in 1882, Antarctic stations must be reached. Not deterred, however, by this difficulty, and remembering doubtless that British seamen were not altogether without fame as Antarctic explorers, he boldly advocated the occupation of Antarctic stations. The whole region, he said, 'should be reconnoitred some years before the transit,' for 'the future astronomical public will not be satisfied unless all practicable use be made of the transits of Venus in 1874 and 1882.'

In 1864 these suggestions were renewed, special attention being directed to Sabrina Land and Repulse Bay. In May 1865, the Astronomer Royal heard that the Geographical Society was endeavouring to move Government to send an expedition towards the North Pole, and he immediately put in a plea for an Antarctic expedition. 'In the year 1882,' he said, 'a transit of Venus over the sun's disc will occur; the most favourable of all phenomena for solution of the noble problem of determining the sun's distance from the earth.' He then stated that the southern stations must be on the Antarctic continent, and pointed out that although, if such an expedition were undertaken the astronomical observations must take precedence of all others, 'there would be no difficulty in combining with them any other inquiries of geography, geology, hydrography, magnetism, meteorology, natural history, or any other subject for which the localities are suitable.'

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[1] One observer, at Caen, using a very small telescope, found the interval between real and apparent contact to be more than two minutes and a half—'a monstrous cantle out,'

But it was in December 1868 that the suggestions for Antarctic reconnaisance first took definite form. Then did the Astronomer Royal marshal an array of naval authorities —Admiral Richards (Hydrographer to the Admiralty), Admiral Ommanney, Commander Davis (a companion of Sir James C. Ross in his celebrated Antarctic expedition), Captain Toynbee, and others—in support of the schemes which during the preceding eleven years he had from time to time advocated; and, with excellent unanimity, these authorities expressed their belief that Antarctic explorations could be usefully and safely carried out.

Hitherto the astronomers of other countries had taken no part in these preliminary inquiries and suggestions. It was doubtless felt that the matter could be well left in the hands of so excellent a mathematician as Sir George Airy. We find, in fact, that in the communication addressed in 1868 to the Astronomical Society, the part which other nations were to take was indicated as well as that which our country might regard as specially her own. The lion's share was taken indeed for England, which was to occupy in 1874 all the four regions suitable for observing by Delisle's method, while in 1882, besides taking her share in applying this method, she was to occupy stations on the Antarctic continent. It is rather singular that no part whatever was assigned to America until 1882, when 'the utmost reliance might be placed on the zeal of our American brethren' for observing the transit of that year at stations in the United States.

But the next few years saw all these ideas changed. It was found that an error had been made in the original investigation of the conditions of the two transits, and that it is for the later not the earlier transit that Halley's method fails. Accordingly, ample preparations were made for observing the duration of the transit of 1874 from suitably selected stations. Among these were several which had been already chosen for the other method; but others were new. In particular the Russian Government provided for a whole range of stations in Eastern Siberia where the transit had a lengthened duration, while America, France, and Germany arranged to occupy Crozet Island, St. Paul's Island, Campbell, Auckland, and other islands in the sub-Antarctic Seas, which, with Rodriguez, Kerguelen, and other places to be occupied by England, formed ample provision for the observation of the shortened duration.

A little disappointment was occasioned to those who may have hoped, from the plans published in 1868, that Antarctic exploration would have been undertaken for observing the transit of 1882. So soon as it was pointed out that no good could result from the occupation of Antarctic stations in that year, all those plans were very properly abandoned. It had now become known, however, that Antarctic stations would be more useful during the transit of the present year than it had been supposed they would be in 1882. Some imagined that the authorities who had been so enthusiastic in favour of Antarctic exploration for one transit would not be altogether opposed to such exploration for the other. This hope was doomed to be disappointed. In fact the Astronomer Royal and the Admirals grew quite facetious in ridiculing the idea of Antarctic exploration; though they suddenly became serious, even to severity, when reminded of the views they had themselves expressed in 1868. Thenceforth they deprecated jesting with a touching solemnity.

But after all, Antarctic exploration was not a point of great importance for the transit of 1874. So admirably was the method of durations suited for this transit, that without incurring the dangers of Antarctic voyaging—whether these dangers be excessive, as now stated by the Admiralty, or slight, as they stated in 1868—a large number of stations could be occupied both in the northern and southern hemisphere, whence the whole transit could be seen. And fortunately for science the opportunity was recognised early enough to be turned to good account. Russia, as we have stated, occupied no less than eleven northern stations for observing the whole transit, America, Germany, and France occupying between them seven or eight others in Siberia, North China, and Japan, while England occupied one in North India.

In the southern hemisphere nearly all the stations were such that the duration of the transit could be observed, except Cape Town, which had special value as a station for observing the middle of the transit. England occupied four stations in the southern hemisphere, besides Cape Town, Melbourne, Sydney, and other places already provided with astronomical instruments; and America, France, and Germany occupied many other southern stations for applying Halley's method. But it is not by any means to be supposed that Delisle's method was neglected. England, for instance, occupied the Sandwich Islands where only the ingress could be observed, and the Isthmus of Suez whence only the egress could be observed, and Russia had a yet larger number of astronomers devoted to the observation of egress only. Moreover, all the stations whence the duration could be seen were excellent stations for observing ingress and egress alone, so that where bad weather unfortunately prevented the observers from noting the duration, they still had a chance of doing useful work.

This, in fact, was one great reason why it would have been little less than a disaster for science had the value of the transit for Halley's method not been noted in good time; because it was hardly to be expected that other nations would occupy second-rate Delislean stations when England and Russia had all the best stations of that kind, whereas under the actual circumstances a large number of second-rate but excellent Delislean stations were occupied because they were first-rate Halleyan stations.

STAR-CLOUDS AND STAR-MIST.

THERE are some scientific questions which have excited an interest seeming at a first view disproportioned to their intrinsic importance. Sometimes the distinguished position of those who have taken part in scientific discussion, at others the skill and acumen with which rival theories have been maintained, have directed exceptional attention to particular questions. Too often personal animosities have become associated with a subject of discussion; for unfortunately it sometimes happens that

> The man of science himself is eager for glory and vain,
> An eye well practised in nature, a spirit bounded and poor.

But there have also been occasions when the singular progress of a dispute, the swaying hither and thither of contending evidence, and even the repeated apparent settlement of the question, by what seems like overwhelming evidence on one side or the other, have given the discussion a singular (perhaps a factitious), and in a sense an almost romantic interest.

Among questions of this sort, few have been more interesting than the subject of which I propose to give now a short sketch, partly historical, partly explanatory—the history running up to this present time, the explanation to be completed only hereafter, and perhaps at a very remote date, if at all.

When the depths of the heavens are explored with a powerful telescope a number of strange cloud-like objects are brought into view. It is startling to consider that if the eye of man suddenly acquired the light-gathering power of a large telescope, and if at the same time all the single stars disappeared, we should see on the celestial vault a display of the mysterious objects called nebulæ or star-clouds, exceeding in number all the stars which can now be seen on the darkest night in winter. The whole sky would seem mottled with these singular objects. With reference to nebulæ, or rather to certain classes of them, opposite views were for a long time maintained by astronomers. 'Whether,' said Humboldt, half a century since, 'the cloudy masses referred to be indeed composed of luminous matter, or whether they are merely remote, closely crowded, and rounded clusters of stars, is a question which has for more than two hundred years been agitated among astronomers.' Galileo and Kepler and Halley discussed the question when as yet the nebulæ might be counted on the fingers of one hand. I might, indeed, even go further back, and quote the views of Tycho Brahe, who wrote before the telescope was invented. Later, the Cassinis and Mitchel, on one side. Durham, Lacaille, and Kant, on the other, supported the rival theories according to the evidence available in their day. Then came the wonderful labours of Sir William Herschel, who in an age when nebulæ had been counted by tens, continued to send in to the Royal Society lists of thousands of these objects. His discoveries attracted fresh attention to the subject of controversy. Holding first the opinion that all nebulæ are star-clouds—that is, closely aggregated congeries of stars, reduced by distance to the appearance of clouds—Herschel came round in the latter part of his career to the contrary view, and gradually, after many disputes among his followers and opponents, the question was held to be settled in favour of the opinion he maintained. How, since then, a contrary opinion gradually struggled into favour, and was eventually established, as was thought, upon the firmest possible basis; how this opinion, at the moment when it seemed that all astronomers were about to accept it, suddenly and unexpectedly received the *coup de grâce* from an eminent physicist of our day; and how later the simple interpretation, which seemed thus suggested, has been in turn rendered doubtful by fresh evidence, is what I now propose to explain.

It will not be uninteresting, however, to examine briefly in the first place some of the more striking features of the wonderful universe of nebulæ. There are, indeed, few subjects in astronomy better calculated to excite emotions of astonishment and awe than those which are associated with the remotest depths of space yet reached by the telescope. The feeling with which men in all ages regarded the star-lit vault of heaven is far less intense than that with which the astronomer gazes into depths 'to which the visible skies are but as the portal,' and analyses the fantastic star-clouds which come into view with each increase of telescope power. 'System on system of majesty unspeakable, float,' said the late Professor Nichol, 'through the fathomless ocean of space. Our galaxy, with splendours that seem illimitable, is only an unit among unnumbered throngs; we can think of it, in comparison with creation, but as we were wont to think of one of its own stars.' But now a yet more startling view of the habitudes of space is presented to our contemplation. Recognising the possibility that some nebular objects which the telescope reveals to us may be systems of stars external to our own, and in a sense resembling it, we are yet forced to recognise the existence of vast wildernesses of matter presenting no characteristics such as we had become accustomed to. We learn to recognise in our own stellar system a far more varied structure than we had hitherto supposed it to have; while as respects external galaxies, not new systems merely, but new kinds of systems are revealed, while the imagination is left to picture yet more wonderful systems of systems within depths to which the most powerful telescopes yet made by man cannot penetrate.

It was early recognised by Sir William Herschel that the nebulæ admit of being arranged into certain very distinct classes. There are the star-clusters, splendid gatherings of stars, the most striking perhaps of all telescopic objects. Some of these are so magnificent that what Professor Nichol said of them is probably not far from the truth, viz.: 'That no one has ever seen them in a telescope of adequate power without uttering a shout of wonder.' Then, as though by mere increase of distance, we have star-groups of less splendour, their constituent orbs more and more closely congregated, until at length we either reach depths at which it is no longer possible to distinguish separate stars, or else we examine regions where the stars are in reality so much smaller and so much more closely set that they seem to form mere aggregations of star-mist. Among such telescopic objects are to be seen nebulæ whose characteristics suffice to show that could we only increase the power of our telescopes we should discern the separate stars now lost to us under a milky haze. A sparkling appearance which the practised observer cannot fail to recognise indicates that the source of the light consists of a multitude of brilliant orbs, and not of a luminous haze or mist.

Respecting the important classes of nebulæ just spoken of, but one opinion prevailed till recently among astronomers. These clusters have been held to be so many distinct aggregations of stars scattered throughout the depths of space at various distances, many of them comprising within their bounds a far larger number of orbs than the naked eye perceives upon the darkest and clearest night. The same opinion was also formed of those objects which, in the magnificent telescope of Lord Rosse, have been found to present a spiral or whirlpool appearance. Indeed, there were those who looked upon our galaxy as being a member of the same class, so that to astronomers in those outlying universes the Milky Way, and all the stars which deck our nocturnal skies, would seem reduced to a small spiral coil of hazy light. Whether this opinion, or the general view which associates the stellar cluster with a theory of external galaxies, be correct, it is not here my special purpose to inquire. I mention the view because it for a long time held undisputed sway among astronomers, and because it conveniently illustrates the aspects of certain classes of nebulæ.

What was disputed among astronomers, even in the time when the theory just described was accepted, was the question whether we can pass from the forms of nebulæ above described to those which remain to be considered, or whether a broad line of demarcation separates the latter entirely from the former, and forces us to look upon them as objects of quite another kind.

In searching over the heavens Sir William Herschel came across certain objects which might very well at first sight be mistaken for tailless comets, or even for enormous planets removed to so great a distance from the sun as to shine with a very feeble light. He called these objects planetary nebulæ. As described by him they presented a most perplexing subject of inquiry. 'They are,' he wrote, 'somewhat extraordinary objects, with round or slightly oval disks, in some instances quite sharply terminated, in others a little hazy at the borders, and of a light exactly equable, or only a very little mottled, which in some of them approaches in vividness to the light of actual planets.'

It was principally the examination of these strange objects that led Sir William Herschel to abandon the view that all nebulæ are composed of stars. It was the peculiarity of this admirable astronomer that he added to extraordinary skill as an observer an acumen in the interpretation of observations which has never perhaps been equalled. I may notice in passing that the two qualities are seldom found in combination, at least in their highest excellence. Observers of the utmost eminence might be named who have been wholly wanting in the power of drawing from their observations the inferences justly deducible from them. And on the other hand, our leading theorisers in astronomy have seldom shown any considerable, or even moderate, powers of observation. It may undoubtedly be ascribed to the union of the two qualities in their highest excellence in Sir William Herschel, that his labours have influenced in so remarkable a manner the progress of modern astronomy, and that the praise bestowed upon him of having broken down the barriers of the heavens (*cœlorum perrupit claustra*, says his epitaph in Upton churchyard) has not been looked upon as exaggerated.

After duly weighing all the circumstances connected with the planetary nebulæ, this great astronomer at length came to the conclusion that they should rather be looked upon as presenting the matter from which at some distant epoch a single star is to be formed than as consisting of a multitude of stars removed to a very enormous distance. Strange as the idea may seem at first, it does not seem improbable when all the circumstances are considered. If we look at our sun and his attendant system of planets, and note the brilliancy of his light, the apparent density of his mass, and the solidity of the orbs which circle around him, it will indeed appear amazing that any could be found so bold as to associate that system with a faintly luminous globe of gaseous matter such as Herschel supposed a planetary nebula to be. In every aspect, in brilliancy, in density, in solidity, the contrast seems most striking, but when we come to consider the enormous volumes of the planetary nebulæ, we find that on the lowest possible estimate of the density of the gas supposed to form them, they must contain an amount of matter fully equal, in many instances, to that contained in the whole solar system; and that, therefore, we cannot doubt that from their condensation solid bodies as large as our sun, or larger, might very well be constructed.

A few facts drawn from the calculations of the younger Herschel will suffice to show that this conclusion is just. There is to be found in the constellation of the Water-bearer a planetary nebula with respect to which it has been shown that supposing it not farther from us than the nearest fixed star, its globe would fit into the enormous orbit of distant Uranus, somewhat as a terrestrial globe within its brazen meridian. It follows that this nebula, at the lowest computation—for no one doubts that the nebulæ are far beyond the nearest fixed star—has a volume exceeding the sun's nearly seventy thousand millions of times. It would follow, of course, that the matter of which it is made must be lighter than that which forms the sun, in an exactly corresponding proportion, if this nebular globe and our sun exactly counterpoised each other. Matter such as this would be fifty million-fold lighter than common air! One peculiarity of the planetary nebulæ is very striking. It has been observed that whereas among the single stars a bluish or greenish tint is never noticed, all the coloured stars being red, orange, or yellow, several of the planetary nebulæ are characterised by a very distinctly marked tinge of bluish green. We shall presently see the meaning of this peculiarity.

For reasons which will appear in the sequel, I mention at this stage certain nebulæ which astronomers associated formerly with the planetary nebulæ. These are the singular objects called ring-nebulæ. In these we see a well-marked circular or oval ring of light surrounding a space which is either wholly dark or illuminated with a fainter light than the enclosing ring. We must also include in the same category the strange object called the dumb-bell nebula, which, as seen by Sir William Herschel, presented the figure of an oval spot of milky light, enclosing a brighter portion shaped somewhat like a dumb-bell.

The last order of nebulæ is that from which Sir William Herschel derived the strongest arguments in favour of his view that a substance of a truly nebulous character, or *star-mist* as some astronomers named the supposed matter, is diffused in enormous masses throughout universal space. Anyone who looks at the three small stars which form the 'Sword of Orion' (easily recognised by their position beneath the three bright stars of his belt), will notice, if the night be dark and clear, that one of them is involved in a faint nebulous light. That light is the famous nebula in Orion, which has perhaps attracted more notice among observers than any other celestial object, except the sun and perhaps the Saturnian ring-system. In a powerful telescope this nebula is the strangest and most fantastic object that can be conceived. 'Inexplicable to the instructed as to the uninstructed eye,' says one who was privileged to examine the nebula with the highest telescopic power yet placed at man's disposal, 'each can but gaze on those inextricable branches and windings, those cloudy masses thinning off into the veriest shadows of being, or those other bright but comparatively isolated patches lying as it were on the shore of absolute blackness ; yes ! forms so mysterious none can describe, or while they contemplate them imagine description to be possible.' In another place, he says, 'How gorgeous the brighter parts of the nebula ! How countless those streamers branching from it on every side ! How strange, especially, that large horn on the north rising in relief from the bleak skies like a vast cumulus cloud !'

The Orion nebula may be looked upon as a type of the class of objects known as the irregular nebulæ. Extending over a wide range of sky, over a range which indeed grows larger and larger with each increase of telescopic power, these objects incalculably exceed all other known objects in volume, even if we suppose them to be no farther from us than the nearest of the fixed stars. There are not many irregular nebulæ, yet they probably cover a greater extent of the heavens than all the others—six thousand in number together. One in the southern heavens, known among astronomers as the great nebula in Argo, surpasses the Orion nebula in easy visibility. Indeed, from my own observations, I can confirm the statement made some ten or twelve years ago by Major J. Herschel, in a letter from India, that 'the eye catches the nebula as readily as it would the Pleiades.' The statement is the more noteworthy from the fact that when Sir John Herschel surveyed the southern heavens the nebula could not be seen with the naked eye. It must be mentioned, however, that in that day there was a brilliant star shining right through the heart of the nebula, which probably dimmed its apparent lustre. This star, though still in its old place, is now so much fainter as to be barely visible to ordinary eyesight.

It need hardly be pointed out that the enormous nebulæ now considered must at once be placed in a class apart from others, whatever opinion we might form of their constitution. If a star-cluster were to approach so near to us as to cover the same extent of sky as the Orion nebula, it would blaze with a lustre sufficient to convert night into day. On the other hand, if the Orion nebula really consisted of stars, placed at so vast a distance as not to be separately visible, all the systems of stars we have as yet considered would shrink into utter nothingness in comparison with so enormous a universe. A parallel has sometimes been drawn between the great Orion nebula and another known as the Andromeda nebula, and also as 'the Transcendently Beautiful Queen of the Nebulæ.' Each is easily visible to the naked eye on a clear and moonless night, and each, though examined with the largest telescopes ever constructed, resisted (until quite recent times) all the efforts made by observers to resolve them into stars, or at least into discrete points of light. But here the resemblance ceases. The Andromeda nebula is comparatively regular in figure, is very much smaller than the Orion nebula, and there is no marked increase in its apparent extent, as larger and larger telescopes are directed towards it. We have seen that the irregularity of the Orion nebula is its most marked characteristic, and also that there seems to be no limit to its apparent dimensions, each increase of light-gathering power having brought new branches and outlying streamers into view. It was therefore a mistake to suppose that the course of discovery respecting the Andromeda nebula could in any way illustrate the nature of the Orion nebula. Moreover, as we shall see, very decisive evidence has been obtained of a total dissimilarity in the constitution of these objects.

Sir William's Herschel's argument respecting the Orion nebula and the other objects of the same class may be stated as follows : So long as it is possible to do so, we should be led to prefer that interpretation of observed phenomena which corresponds most closely with the analogies we are familiar with. But in the case of the irregular nebulæ we *must* depart from all such analogies. If they are galaxies of stars, their dimensions must exceed those of all other star-systems taken together ; if their dimensions correspond with those of other stellar universes, their constitution must be totally different. Having a choice of two views, each of which involves a total want of analogy with all that is elsewhere to be observed, that view seems most reasonable which does not require us to assign to these objects dimensions altogether inconceivable. For although there is nothing in the universe to be paralleled with the irregular nebulæ, we have instances, even within the range of the solar system, of objects (the comets) which are of a totally different constitution from the rest of the system they belong to. Such, then, the irregular nebulæ may be—aggregations of some unknown form of matter subserving purposes in the economy of nature which are at present altogether unintelligible to us. They may be constituted of the same material as the planetary nebulæ, but whereas these, owing to the regularity of their figure, may be looked upon as undeveloped stars, the wild and fantastic wisps of the irregular nebulæ afford no intelligible evidence of the processes they are undergoing.

During the first quarter of the present century Herschel's views were almost universally accepted among astronomers. The skill with which he had prepared the ground for the theory, and the address with which he drew illustrations of his theory from the number of nebulæ which he had observed, would seem to have forced the new views upon the acceptance of his contemporaries. He pointed to nebulæ presenting every variety of structure, and in every stage of progress, from the stage of simple globes of star-mist up to a state of condensation in which scarcely any nebulosity could be perceived. Nay, he went further, and included our own sun among the number of condensing systems. Viewed from a great distance, he said, there can be little doubt that our sun would appear as a star involved in a nebulous light; for the phenomenon called the Zodiacal Light surrounds the sun on every side to a distance of seventy or eighty millions of miles, and this envelope could present no other than a nebulous appearance.

Time passed, and many theories of Sir William Herschel's, which had seemed bold if not rash at the time of their promulgation, had been established on a sure basis by the labours of his successors. But his hypothesis of star-mist, which had been freely welcomed when it first appeared, had not been of the number. 'Notwithstanding the ingenuity of the illustrations and incontestable force of reasoning by which Herschel sought to establish this bold hypothesis, it has not,' said Professor Grant in 1852, 'received that confirmation from the labours of subsequent inquirers which is so remarkable in the case of many of the speculations of that great astronomer. It is now generally admitted that the changes which at one time were supposed to be taking place in some nebulæ were altogether illusive, having been suggested partly by erroneous delineation of the objects as they actually appeared in the telescope, and partly by the different aspects which they assumed when viewed in telescopes of different degrees of power.'

It was undoubtedly to the revelations afforded by Lord Rosse's great reflector that the disrepute into which Herschel's views had fallen at this time was chiefly due.

In the first place, the planetary nebulæ were found to present quite a different aspect in the Parsonstown reflector from that shown by the Herschelian four-feet mirror. There is one picture in works on astronomy which attracts the attention of the least thoughtful. A strange nebula is presented in it, resembling in appearance the face of some grotesque animal glaring through a species of halo. This object is no other than one of Herschel's planetary nebulæ, as it appears under the wonderful illuminating power of the great reflector. Equally remarkable changes are observed in other instances; and in fact, Lord Rosse stated that 'in every instance examined save one the planetary nebulæ have been found to be nebulæ with hollow centres.' By resolving the great ring-nebula in Lyra into stars—or what appear to be such—another blow was dealt to the nebular hypothesis.

But the main contest, as might be expected, was carried on around the nebula in Orion. Here the supporters of Herschel's views found their chief stronghold. One nebula after another had been resolved into stars beneath the penetrating gaze of the great reflector. News came across the Atlantic that the magnificent refractor of the Cambridge Observatory had, in the clear American skies, reduced the great Andromeda nebula into discrete stars. Yet still, so long as the Orion nebula remained unresolved, it was impossible to oppugn successfully the views of Sir William Herschel. 'With an anxiety natural and profound,' says Professor Nichol, 'the scientific world watched the examination of Orion with the six-feet mirror, for the result had either to confirm Herschel's hypothesis, in so far as human insight could confirm it, or unfold among the stellar groups a variety of constitution not indicated by those in the neighbourhood of our galaxy.'

At length the news came that the nebula had yielded. The first account of the resolution of the nebula was conveyed in a letter from Lord Rosse to Professor Nichol. 'I think I may safely say,' he wrote, 'that there can be little, if any, doubt as to the resolvability of the nebula. Since you left us, there was not a single night when, in the absence of the moon, the air was fine enough to admit of our using more than half the magnifying power the speculum bears. Still, we could plainly see that all about the trapezium is a mass of stars; the rest of the nebula also abounding with stars, and exhibiting the characteristics of resolvability strongly marked.'

And thus, said astronomers, doubt and speculation vanish from this subject for ever. The new fact proves that to be real which Herschel deemed incomprehensible. 'And now,'—again I quote Professor Nichol, for indeed I am here dealing with what others have said, and not with actual facts—'*now* the astronomer can adduce no justification of the assertion that any nebula, however stubborn, ought to be interpreted contrary to the analogy of all other known objects of its kind.'

So firmly did the persuasion become fixed in astronomers' minds that Herschel had been mistaken, that it is almost impossible to take up a book on astronomy written between the years 1848 and 1864 without finding attention called to the new views which had replaced those he had upheld. There were one or two, amongst others the late Admiral Smyth, who thought the change of view over-hasty; but in the general rush of opinion against Herschel's theory of star-mist, the voices of those who still supported it were lost.

On a sudden Herschel's opinion was justified. It had taken long years of patient observation to overthrow his theory, but it was restored to favour by an observation which did not occupy five minutes. It is to the spectroscope, the most wonderful instrument of modern research, that we owe the complete rehabilitation of Herschel's theory of star-mist. A very few words will explain the whole matter to readers who remember the three fundamental laws of this new mode of investigation, viz.: that first, light from a burning solid or liquid source gives the rainbow-coloured streak of light commonly known as the *prismatic spectrum*; secondly, when vapours surround such a source of light, the rainbow-coloured streak is crossed by dark lines; and thirdly, when the source of light is gas, there is no longer a rainbow-coloured streak, but merely a finite number of bright lines.

Dr. Huggins had been engaged with Professor Miller in a careful spectroscopic examination of the brighter fixed stars. These physicists had succeeded in establishing a series of facts more interesting and surprising, perhaps, than anything which had before been revealed to man.

It occurred to Dr. Huggins that if the new analysis could be applied to objects so faint as the nebulæ, it could hardly fail to afford important revelations respecting their structure.

The apparatus he made use of was one constructed by Mr. Browning, F.R.A.S., the optician, with the express object of giving a spectrum of a distinctly marked and brilliant appearance. The nebula selected for observation was a small planetary one in the Dragon. I quote with a few verbal alterations Dr. Huggins's own account of the result: 'When I had directed the telescope armed with the spectrum apparatus to this nebula,' he wrote, 'I at first suspected that some derangement of the instrument had taken place; for no spectrum was seen, but only a short line of light. I then found that the light of this nebula, unlike any other extra-terrestrial light which had yet been subjected by me to prismatic analysis, was of definite colours, and therefore could not form a spectrum. A great part of the light is monochromatic, and so remains concentrated in a bright line occupying a position in the spectrum corresponding to its colour. Careful examination showed a narrower and much fainter line near the one first discovered. Beyond this point, about three times as far from the first line, was a third exceedingly faint line.'

Here, then, at once was the most absolute proof that Herschel had been perfectly justified in supposing that the planetary nebulæ are gaseous. Nothing but a luminous gas can give a spectrum of bright lines; and therefore, so far as this question was concerned, no further inquiry was required.

But before passing on to mention the results obtained when other nebulæ were examined, we must inquire what is the gas of which this nebula is composed? Had any other mode of inquiry but spectroscopic analysis been employed, it would have been idle to ask such a question; but the wonderful powers of the analysis are equal to answer even such a question as this. From the position of one of the bright lines, it is inferred that the gas nitrogen is one of the constituents of the nebula ; another line indicates the existence of the gas hydrogen in that far-off system ; the third line has not yet been associated with any known terrestrial element, though it is near one belonging to the metal barium and still nearer to one belonging to oxygen ; a fourth line occasionally seen belongs to hydrogen.

Dr. Huggins examined a large number of the planetary nebulæ, obtaining in each case a spectrum which indicates gaseity. In some cases only one line could be seen, in others two, more commonly three, and in a few instances four. When these lines were seen they invariably corresponded in position with those already described. The single line sometimes seen corresponded with the brightest line of the three ; and when a second line was visible, this also was no new line, but agreed with the second brightest line in the three-line spectrum.

The fourth line was first seen only in the spectrum of a very bright small blue planetary nebula, but was later observed in other cases, and especially in the great Orion nebula.

So far we see that Sir William Herschel's views have been abundantly justified. The planetary nebulæ of which he said that they are most probably gaseous have been shown to be so. The Orion nebula, respecting which he expressed a more certain opinion, and on which he founded so confidently his much-vexed theory of star-mist, has at last supplied confirmatory evidence of Herschel's acumen. When Huggins was observing the planetary nebulæ, Orion was not then visible at night. It was not until several months had passed that he was able to apply his spectroscope to the analysis of this famous object. We may imagine that he came to the examination of the spectrum with some anxiety as to the result. 'The telescopic observations of this nebula,' he says, 'seem to show that it is suitable to a crucial test of the usually received opinion, that the resolution of a nebula into bright stellar points is a certain indication that the nebula consists of discrete stars.' A simple glance resolved the difficulty. The light from the brightest part of the nebula, the very part which under Lord Rosse's great reflector blazed with innumerable points of light, gave a spectrum identical in all respects with that which Huggins had obtained from the planetary nebulæ. Thus, what had been deemed boldness in Herschel—namely, that he should have associated the wildest and most fantastic nebula in the heavens with the circular and (in ordinary telescopes) almost uniformly luminous planetary nebulæ—was unexpectedly confirmed, after the stellar theory of the great nebula had been maintained for years, and apparently established by direct observation.

There, for several years, the matter seemed to rest. In 1867, it is true, the present Lord Rosse announced that parts of the Orion nebula seemed manifestly stellar in constitution; but the general result that the great nebulous mass is gaseous seemed hardly to be shaken.

But within the last two years a series of observations have been made, marvellous in their nature and promising to lead to results throwing entirely new light on the great nebula in Orion. A method of research which in former times would have seemed utterly impossible—its very conception so wild and fanciful as to resemble the dream of a distempered mind—has been applied, which promises to give in the first place far more perfect views of the nebula than any yet obtained, and then to give a searching system of analysis by which the nebulæ may be in the most effective manner examined piecemeal, so that varieties in the constitution of its various parts may be recognised, if any such exist.

What would have been thought, even by such men as Sir W. Herschel, Laplace, or even Newton, had the idea been suggested by which a prepared plate should replace the retina of the eye, and instead of the ordinary time for vision, an hour or two hours or more should be employed in receiving on that artificial retina a picture of a faintly luminous celestial object? If the idea could have been for a moment regarded as more than a wild and visionary fancy, what would even such men have thought if it had been further suggested that the artificial eye thus prepared might be employed to *see* rays which the ordinary eye can never see, nay, even to determine the nature of the substance from which such rays come?

Not to deal further with the wonderful *nature* of the photographic method applied to the investigation of the Orion nebula—a subject which requires full treatment separately, on account of its own intrinsic importance—I proceed to inquire what this method has revealed in regard to the question whether the great nebula is or is not a mass of self-luminous vapour.

Dr. Henry Draper, of New York, sent over to England, some two years since, enlarged positives from a negative on a square inch of glass, in which a wonderful amount of detail was shown. In the enlarged positive the exceedingly delicate details, clearly discernible in the negative, could not be perceived, and although, even in these imperfect views the features of the nebula could not possibly be mistaken, some persons in this country who had tried but failed to accomplish what Dr. Draper had achieved, were not above asserting that the markings in the views sent arose simply from imperfections in the plate, which chanced to have an arrangement resembling that of the luminous masses which form the Orion nebula. Recently, however, Dr. Draper has taken the only worthy revenge he could take for these petty insults; he has obtained still better negatives (for one of them the exposure lasted no less than two hours and seventeen minutes), from which more satisfactory enlargements have been formed —and now we have photographs of the nebula which even the most envious of unsuccessful rivals have been obliged to accept as valid representations—albeit the negatives themselves are far better, nay, are altogether more correct representations of the nebula than the finest drawings yet made even with the most powerful telescopes.

Wonderful, however, though this is, a far more important achievement is one which Dr. Draper and Dr. Huggins have obtained with equal success—the photographing of the spectrum of the great nebula. For thus not only is an observation in effect made which can be repeated, as it were, by everyone who examines the photographed spectrum, but more is thus shown than the eye can see, and moreover peculiarities distinguishing one part of the nebula from another can be recognised, which, though they might be perhaps discernible by the eye, could hardly be regarded as demonstrated but for the use of this self-recording method.

Now here, strange to relate, we get evidence by which the conclusion which had been generally accepted is shown to be not altogether just. The nebula is seen to be in great part gaseous, and where gaseous to shine in the main with the tints described above; but parts of the nebula are not gaseous, and those portions which are so are not all constituted in the same manner. For both Dr. Draper and Dr. Huggins find portions of the nebula, and especially the more condensed parts which lie to the left (in the ordinary telescopic view of the nebula) of that portion which is called the Fish's Mouth, to give a continuous spectrum—in other words, the same spectrum which we obtain from a star,[1] or a star-cluster. This is the spectrum arising from a glowing solid or liquid mass, or if from a gaseous body then the gaseous body must be in a state of great compression. For my own part, indeed, I believe that the rainbow-tinted spectrum derived from the Orion nebula by no means indicates that the source of light is other than gaseous—so that we may describe the condensed parts of the nebula in Orion as shining with lustre indicating stellar or sun-like constitution. We may then find here the explanation of the bright points of light seen by Rosse in the brighter parts of the Orion nebula—in these parts the gaseous matter would seem to have already aggregated into stars or suns.

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[1] I say here the same spectrum, though, strictly speaking, the spectrum of a star is not continuous, but consists of a rainbow-tinted streak crossed by dark lines due to absorption by the stellar atmosphere. But with the open slit used by Drs. Draper and Huggins to obtain the photograph of the spectrum of the Orion nebula, such dark absorption bands could not possibly be seen. They are not seen, in fact, in the spectra of star-clusters.

But the stars thus forming must be immersed in the glowing gas forming the general substance of the nebula. We might infer this independently of observations tending actually to prove it. For it would be absurd to suppose that the nebula is a flat surface, constituted in one way in one part and otherwise in another. We must always remember this in observing objects like comets, nebulæ, star-clusters, and so forth. We have a view of them from one side. Viewed from any other direction they would appear differently, no doubt; but they would still present the same general characteristics. We often find them spoken of as if they were regarded as flat objects which we see from in front, or square to their surface, so that if viewed edgewise they would appear as mere lines. But, when we find parts of the nebula in which stars seem to be congregated, with nebulous matter above, below, and on either side in the field of view, we must remember that nebulous matter lies also in all probability (certainly, one might fairly say) between us and the stellar aggregation, as well as on the farther side.

As evidence of this, I may note that when in Dr. Draper's photographic experiments the slit of the spectroscope was placed across the trapezium, one of the hydrogen lines shown in the spectroscope was of the same length as the slit, a duplication of effect being noticed where it intersected the continuous spectrum of the trapezium stars. On this Dr. Draper remarks that, 'if this effect is not due to flickering motion in the atmosphere' (an explanation which may be at once dismissed, as the effect would certainly have been recognisable in that case all along the bright lines), 'it would indicate that hydrogen gas was present even between the eye and the trapezium.'

Varieties of gaseous constitution are also indicated in different parts of the nebula. 'In the case of two other faint lines in this vicinity,' says Dr. Draper, speaking of the brighter part of the nebula, 'I think the lines are not of the length of the slit, one being quite short and the other discontinuous. If this observation,' he proceeds, 'should be confirmed by future photographs of greater strength, it might point to a non-homogeneous constitution of the nebula, though differences of intrinsic brightness would require to be eliminated.'

Again, variety is indicated (a different kind of variety) by the differences between the photographs obtained by Dr. Draper and Dr. Huggins. Speaking of a bright line, not visible in the ordinary spectrum (belonging in fact to the ultra-violet portion), which is shown conspicuously in Dr. Huggins's photographs, Dr. Draper remarks that he has not found that line, though his photographs show other lines which Dr. Huggins does not appear to have photographed. Dr. Huggins says, indeed, of other lines which he expected to find, 'if they exist in the spectrum of the nebula, they must be relatively very feeble : I suspect, indeed, some very faint lines in this part of the spectrum' (between the most refrangible of the lines visible to the eye and the line just referred to), 'and possibly beyond the more conspicuous photographic line. I hope by longer exposures and with more sensitive plates to obtain information on this and other points.' Dr. Draper considers, and it seems to me justly, that the difference between the results obtained by himself and Dr. Huggins may be due to the fact that the slit had been placed on different regions of the nebula, though part of the difference may have resulted from Dr. Huggins's employment of a reflector and a prism of Iceland spar, whereas Dr. Draper used a reflector and a prism of flint glass.

It is manifest that we have in the application of photography to the gaseous nebulæ a new and potent means of research. A remark applied by Dr. Huggins to the use of photography in obtaining star-spectra, may probably be applied with even more force here. 'We shall, perhaps, underrate,' he says, 'the importance of a knowledge of the ultra violet' part of spectra, 'if we regard these photographs as simply adding so much in length to the visible spectrum; for there are reasons why a knowledge of this part of the spectrum may be of exceptional value to us.' If, as is probable, the luminosity of the gaseous portion of the Orion nebula is accompanied by but a relatively small proportion of heat, then the rays from the violet and ultra-violet part of the spectrum are likely to give us much more complete information respecting the constitution of these nebulous masses than can be derived from the visible part of the spectrum. Still, it is well to remember that should the ultra-red part of the spectra of nebulæ be able to convey information supplementing or adding new force to information otherwise derived, we may entertain good hopes that that part also of the spectrum may be effectively studied. The researches and discoveries of Captain Abney, though they have not yet shown how the ultra-red rays may be used as effectively (for rapidity of action, &c.) as the ultra-violet, have at least shown that they can be used; and we may well believe that the methods which, even now, when this department of the photographic art is in its infancy, have enabled Captain Abney to photograph a kettle of boiling water in the dark by means of its invisible heat radiations, may hereafter be developed to give photographs of the ultra-red spectra of stars, and to show lines (if such exist) in the ultra-red part of the spectra of the gaseous nebulæ. When we remember, however, that there are no visible lines in the yellow, orange, or red, we may reasonably doubt whether any exist in the ultra-red.

Certainly Dr. Huggins's remark seems thoroughly justified by what has been already done in this direction. 'It is not, perhaps, too much to hope,' he says, 'that the further knowledge of the spectrum of the nebulæ afforded us by photography, may lead, by the help of terrestrial experiments, to more definite information as to the state of things existing in those bodies.'

The inquiry may prove to be far more wide-reaching than those might imagine who view it merely as relating to the question how the gaseous or partly gaseous nebulæ are constituted. It may well chance, as long since suggested by Prof. Clark of Cincinnati, and as more cautiously hinted by Dr. Huggins, that in the varieties of constitution observed in the irregular nebulæ and the evidence such varieties afford of progressive change, we may find not merely direct evidence of the development of suns and sun-systems from great masses of nebulous matter (the luminous star-mist of the Herschelian theory), but even what would be a far more important and impressive result —actual evidence of the development of the so-called elements from substances really elementary, or, at any rate, one stage nearer the elementary condition than are our hydrogen, nitrogen, oxygen, carbon, and so forth. The peculiarity of the spectral indications of the presence of nitrogen and hydrogen in the nebulæ is, that only one line of nitrogen and two or three lines of hydrogen are discernible, instead of the complete spectrum of either element as seen under any known conditions, seems suggestive of what may be called a more elemental condition of hydrogen and nitrogen. If it should prove hereafter that in different parts of the great nebula of Orion (or the inquiry may be more successfully pursued with the great nebula in Argo), hydrogen and nitrogen can be traced in gradually varying forms, up to those giving the complete spectrum at some known pressure and temperature, we should not only be able to form some conception of the actual condition of various portions of the great nebulous mass, not only be able to recognise something as to the progressive aggregation of different portions of the mass into suns and sun-systems, but we should learn something as to the conditions under which hydrogen and nitrogen, and perhaps other so-called elements, are formed. Nor can I doubt that, should this prove to be the case, the inquiry would presently be found associating itself with those other inquiries by which Rutherfurd, Secchi, Draper, Huggins, and others have been led to recognise the existence of progressive stages in the development of suns themselves. If, for instance, the stars can be arranged in a series whose first term is represented by the blue-white stars, of which Sirius and Vega are the type; the second by yellowish stars, of which Capella and our own sun are typical; the third by orange stars, like Arcturus; and the fourth by red stars; and if of these the bluish-white are the youngest (in development, of course; we speak not of absolute age), and the others more and more advanced, we may well believe that the careful study of objects like the Orion nebula, with the new means now available, may bring us to the knowledge of yet earlier terms in the series, indicating the various steps by which gaseous matter aggregates into embryonic suns, these into bantling orbs, which develope (as time-periods, measureless by man, pass onwards), into suns like Sirius and Vega in growth, and thence to the condition which our own sun has attained.

In conclusion, I would note how abundantly the diverse views, about which I spoke at starting, and the inquiries to which those rival views led, have justified Herbert Spencer's teaching that no answer was ever yet given by science which did not lead to new and closer questioning. On one side and on the other the controversy swayed as fresh evidence on either side was obtained; but science was not content at any stage of the inquiry to 'rest and be thankful.' Now, when so much new knowledge has been obtained, and when so many doubtful points have been disposed of, we are further than ever from actually understanding the mystery of the great gaseous nebulæ. But if we find more than ever about which we are in doubt, we see more than ever how much fresh knowledge may be hoped for as we push forward new inquiries.

HERBERT SPENCER'S PHILOSOPHY.

There are many who find a difficulty in understanding how the philosophy of Mr. Herbert Spencer is related to the Darwinian theories of biological evolution. Many, indeed, seem to find difficulty in recognising at all the nature of the teachings of Mr. Spencer, and especially in determining the position which they hold in modern thought. Some appear to imagine that his views are entirely sociological, others suppose that they involve simply an extension of the Darwinian doctrine to the universe at large, while yet others (as I have repeatedly noticed in converse with those whom I have met during my lecturing tours in this country, America, and Australasia) appear to regard Mr. Spencer as chief among the opponents of religion.

It should hardly be necessary to say that all these views are erroneous; yet knowing as I do, how few there are who have formed any just conception of Mr. Spencer's philosophy, especially in this country (for he is much better understood and appreciated on the other side of the Atlantic), I have seen, somewhat gladly, that certain unfair treatment in 'Reminiscences chiefly of Oriel College,' by the Rev. Thomas Mozley, has led to the publication by Mr. Spencer of a succinct statement of the cardinal principles involved in the successive works which Mr. Spencer has published. The statement is a mere summary, technically, and in some places, rather obscurely worded; but it is of great value, as showing not only what Mr. Spencer has actually taught, but what it has been his special purpose to teach. I propose now to translate the successive items of this statement into more familiar language (in each case giving Mr. Spencer's actual words in the first instance). As, however, the significance of a statement of this kind must always in part depend on the circumstances which elicited it, I deem it well briefly to sketch the matter at issue between Mr. Spencer and the Rev. Mr. Mozley. I do this the more willingly, that, as the former remarks, 'serious injustice is apt to be done by the publication of reminiscences which concern others than the writer of them; and widely diffused as is Mr. Mozley's interesting work, his statement will be read and accepted by thousands who will never see "Mr. Spencer's Rectification."' It appears to me that good service will be done to the cause of justice by helping to spread this rectification as widely as possible.

The passage which has called forth Mr. Spencer's rectification runs as follows: 'I have indulged,' says Mr. Mozley, 'from my boyhood in a Darwinian dream of moral philosophy, derived in the first instance from one of my early instructors. This was Mr. George Spencer, (honorary) secretary of the Derby Philosophical Association, founded by Dr. Darwin,[1] and father of Mr. Herbert Spencer. My dream had a certain family resemblance to the "system of philosophy" bearing that writer's name. There was an important and saving difference between the two systems, between that which never saw the light, and perished before it was born, without even coming to wither like grass on the housetops, and that other imposing system which occupies several yards of shelf in most public libraries. The latter makes the world of life, as we see and take part in it, the present outcome of a continual outcoming from atoms, lichens, and vegetables, bound by the necessities of existence to mutual relations, up to or down to brutes, savages, ladies, and gentlemen, inheriting various opinions, maxims, and superstitions. The brother and elder philosophy, for such it was, that is mine, saved itself from birth by its palpable inconsistency, for it retained a Divine original, and some other incongruous elements. In particular, instead of rating the patriarchal stage hardly above the brute, it assigned to that state of society a heavenly source, and described it as rather a model for English country gentlemen, that is, upon the whole, and with certain reservations.'

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[1] It was 'more than a dozen years,' Mr. Spencer remarks, 'after Dr. Darwin's death in 1802, when my father became honorary secretary. I believe my father (who was twelve years old when Dr. Darwin died) never saw him, and so far as I know, knew nothing of his ideas.'

It will be tolerably obvious that in this passage there is something more than Mr. Spencer—proceeding in his calm way by inquiring rather what others found in it than what he found himself—notes as its purport. It leaves the impression, he says, that the doctrines set forth in the system of Synthetic Philosophy, as well as those which Mr. Mozley entertained in his early days, were in some way derived from the elder Spencer. True, but it leaves also the impression that although the 'brother and elder philosophy had been thus derived,' it owed to Mr. Mozley whatever development it received; he speaks of it plainly as the 'philosophy that is mine.' It conveys very clearly (and also very cleverly) the idea that in Mr. Mozley's opinion the elder philosophy was altogether the nobler and better of the twain, however obvious it may be to sounder judgments that that opinion is altogether erroneous. And then, by saying that even this elder and better philosophy was so palpably unsound that its failure before birth *saved* it from its due fate, it leaves us clearly to understand what a great misfortune, in Mr. Mozley's eyes, has been the birth, growth, and development of the younger and inferior brother. That these palpable sneers (not to say these gross insults) escaped an attention so keen as Mr. Spencer's I do not suppose. It is evident, however, that he very justly regarded them as unworthy of notice—they are, in fact, of the class of innuendoes which may properly be described as womanish (observe, I do not say womanlike). Mr. Spencer directs his whole attention to meet Mr. Mozley's implication that during the last five-and-twenty years he has been allowing himself to be credited with ideas which are not his own. 'Since this is entirely untrue,' he says, 'I cannot be expected to let it pass unnoticed; if I do I tacitly countenance an error, and tacitly admit an act by no means creditable to me.'

He then tells us, in admirably selected terms, just how far he believes himself to be indebted to his father. His indebtedness was general, he says, not special—and indebtedness for habits of thought encouraged rather than for ideas communicated. 'I distinctly trace to him an ingrained tendency to inquire for causes—causes, I mean, of the physical class.' And here let me note in passing, is the great lesson which modern science is ever inculcating. It is here that science influences mental and moral culture most palpably. There is no more valuable safeguard against superstitions of all orders, from those which affect the whole conduct of life, the whole character, down to the paltry superstitions which relate to such matters as helping to salt, walking under ladders, and so forth, than the inquiry always for causes. Breaking a mirror means seven years of sorrow, says the ignorant believer in foolish fancies of the sort: 'In what way? through what relation of cause and effect?' comes the question of common sense, and the notion is at once seen to be an absurdity. If I commit such and such offences, says the believer in a higher form of superstition, I shall be punished; science asks how and why, and in the answer finds the real reason for the moral law. Science finds that offences against right and justice bring always their punishment with them, and shows cause why; establishing thus a sounder and nobler morality than any founded on the merely superstitious fear that some unexplained punishment will fall on us for wrong-doing.

The elder Spencer, says his son, was 'far from having himself abandoned supernaturalism, yet the bias towards naturalism was strong in him, and was, I doubt not, communicated (though rather by example than by precept) to others he taught, as it was to me. But while admitting, and indeed asserting, that the tendency towards naturalistic interpretation of things was fostered in me by him, as probably also in Mr. Mozley, yet I am not aware that any of those *results* of naturalistic interpretation distinctive of my works are traceable to him.'

What is specially noticeable about this part of Mr. Spencer's communication to the *Athenæum*, as bearing on the nature of the Spencerian philosophy is, that he shows Mr. Mozley's statement to be *necessarily* erroneous. The cardinal ideas discussed throughout the series of volumes published by Mr. Spencer were necessarily of much later origin than the period to which Mr. Mozley's account refers. The great generalisation respecting the correlation and equivalence of the physical forces, had not as yet been even thought of. This doctrine and others which are absolutely indispensable elements of the general theory of evolution, were not heard of for years after the time mentioned by Mr. Mozley, so that, unless his statement be regarded as simply untrue, it is manifest that he has (even now, when Mr. Spencer's views have long been before the public) no adequate conception even of the general doctrines of evolution as formulated in our time, far less of those special doctrines with which Mr. Herbert Spencer's name is identified.

Nay, it will be manifest to all who have carefully studied Mr. Spencer's own writings, that he himself had not adopted the views now forming the Spencerian philosophy until long after his father's death. 'In the earliest of them,' he says, '"Letters on the Proper Sphere of Government," published in 1842, and republished as a pamphlet in 1844, the only point of community in the general doctrine of evolution is a belief in the modifiability of human nature through adaptation to conditions (which I held as a corollary from the theory of Lamarck), and a consequent belief in human progression. In the second and more important one, "Social Statics," published in 1850, the same general ideas are to be seen, worked out more elaborately in their literal and political consequences. Only in an essay published in 1852 would the inquirer note for the first time a passing reference to the increase of heterogeneity as a trait of development, and a first recognition of this trait as seen in other orders of phenomena than those displayed by individual organisms. Onwards through essays published in several following years, he would observe further extensions in the alleged range of this law, until, in 1855, in the "Principles of Psychology," it begins to take an important position, joined with the additional law of integration, afterwards to be similarly extended. Not until 1857, in two essays then published, would he find a statement relatively crude in form, of the law of evolution, set forth as holding throughout all orders of phenomena, and joined with it the statement of certain universal physical principles which necessitate its universality. And only in 1861 would he come to an expression of the law approximating in definiteness to that final one reached in 1867.' This, of course, conclusively disposes of the implication in Mr. Mozley's statement; for, were this true, the earlier writings would have contained traces of the doctrines set forth in the later ones.

So much premised, we are prepared to examine Mr. Spencer's summary of the cardinal principles developed in his successive works. So far as the refuting of Mr. Mozley's unfair implication is concerned, nothing more could be needed; but Mr. Spencer thought it might be well to enable the reverend gentleman, who had somewhat rashly attacked him, to compare the propositions of 'the younger philosophy' with that which, because—by his own account—it was unborn at an earlier date, Mr. Mozley has called 'the elder.' The summary was written out some twelve years ago, for an American friend. Let us take the propositions *seriatim.*

1. *Throughout the universe in general and in detail, there is an unceasing redistribution of matter and motion.*

This statement scarcely needs explanation. It means simply that the arrangement of matter in space, whether we consider large masses or small, is ever varying, and that the movements among the different portions of matter, large as well as great, are constantly changing in rate and in direction. In the way of illustration we have a wide field for selection. Amid stellar space stars or suns are rushing hither and thither with motions varying according to the attractions at work in various parts of each star's course. We see that in vaporous matter, or widely distributed cosmical dust, similar motions are continually taking place. The expansive tendencies of gaseous matter are found to be due to the rush of minute molecules in all directions. So that, in the universe of matter as revealed to the astronomer, the same general laws affect the suns which people space, the largest discrete masses we know of, and the molecules which form the intimate substance of gaseous matter, bodies so minute as to lie hopelessly beyond the range of any conceivable increase of microscopic power. In organic matter the same law holds. All the physical processes which are most obvious and familiar, all those which form the subject of the most recondite scientific research, are in reality illustrations of the constant redistribution of matter and motion.

2. *This redistribution constitutes evolution where there is a predominant integration of matter and dissipation of motion, and constitutes dissolution where there is a predominant absorption of motion and disintegration of matter.*

Unfortunately the words in this statement are not altogether well chosen or used in their strictly correct sense—evolution is set against dissolution as if the two were contrasted processes, whereas dissolution is a form of evolution. Moreover, the word 'integration' is not commonly understood, whether in its technical sense (which is almost purely mathematical) or in ordinary language, in the sense in which it is here used. It is rather understood ordinarily to mean the restoration of that which had been made imperfect, than as the converse of disintegration.[1] 'Absorption' also is here used by Mr. Spencer to mean what might rather be described as 'assumption.' The statement may be thus translated into ordinary language :—

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[1] This also is the sense in which the Romans used the word *integratio*. Thus the familar saying of Terence, *Amantium iræ amoris*

This constant change in the distribution of matter and motion, results in some cases in the aggregation into one whole, of portions of matter which before had been apart from each other, the motions of these several portions *inter se* coming to an end, or greatly diminishing, as they thus gather into a single mass. In such cases we have the formation of new masses. In other cases portions of matter which had been aggregated into a single mass are separated from each other, and begin to move freely *inter se.* In such cases we have the dissolution of the masses thus separated into their component parts.

We may select our first illustrations of these converse processes from the celestial spaces, though so far as is yet known these tell us of few cases of dissolution or dissipation, nearly all the processes actually observable being instances of aggregation or of the formation of new masses. We know that in the solar system there are multitudinous systems of meteoric bodies—and we know further that our own earth gathers in many millions of these bodies in each year. The same is doubtless true of the moon, Venus, Mars, and other planets. It must be true in yet greater degree of the sun. Every fall of a meteor is a process of aggregation. When a meteor falls on the earth a new mass is formed out of the two—the earth and the meteor—which had before existed apart. On the other hand, it has been thought possible, some think it has been *proved*, that meteors were originally projected from the sun or planets—if so, every such case was an illustration of the process of segregation; masses which had before formed a single mass, and had shared the same motions in reference to other masses, being thrown apart to move thenceforth for a longer or a shorter period

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integratio est, is properly translated, 'The quarrels of lovers are the *renewing* of love.'

of time (in some cases for ever) independently of each other. If Mr. George Darwin's views are correct, and our moon was at any time separated either in a single mass (or, as I think more probable, a ring of small masses) from the earth, that was a process of dissolution, or, rather, segregation. So also with the other satellite systems, and with the rings of Saturn, if they were really formed in the way suggested. In the latter case these rings illustrate at the same time both processes; for while they were on this hypothesis expelled from Saturn, and so illustrate segregation, their component small masses are gathering together to form hereafter a single moon, or it may be two or three moons, and thus illustrate aggregation.

But to show the generality which characterises Mr. Spencer's theories we may here take an illustration of a quite different character. In the growth of a nation out of tribal races which had been pursuing each a separate existence, nomadically traversing a continent before they settled down to occupy a country, we have a process of aggregation; in the growth of colonies formed out of groups of persons who have left their country at different times and with diverse aims and purposes, we have also a process of aggregation in one aspect, for these colonies are formed by the aggregation of groups before moving apart; but in a wider aspect we have segregation, each colony being separated by the removal of parts from what had before been one whole nation. Where a colony is formed of groups from several nations, there is, in reference to the colony, a predominant aggregation of matter and dissipation of motion; where one nation forms several colonies there is, in reference to the nation, a predominant segregation of matter, and assumption of motion.

But, in fact, as these processes take place throughout the universe in general and in detail, we might select our illustrations from a thousand different sources; we might view matters on a very large scale or on a very small scale, or on a scale having any position between these extremes.

The third and fourth statements of Mr. Spencer's philosophy run thus :—

3. *Evolution is simple when the process of integration, or the formation of a coherent aggregate, proceeds uncomplicated by other processes.*

4. *Evolution is compound when, along with this primary change from an incoherent to a coherent state there go on secondary changes due to differences in the circumstances of the different parts of the aggregate.*

The explanation of 2 leaves little to be explained here. The reader must note carefully, however, that the word evolution here has that limited and incorrect sense in which it has already been used in statement 2. It means here the process of formation of a coherent aggregate out of matter which had before been scattered more or less sparsely. Probably no illustration can be given from nature of an absolutely simple process of aggregation any more than illustrations can be given from nature of perfectly straight lines, circles, ellipses, and so forth. The philosopher may speak of simple evolution and define it, just as the Newtonian may speak of motion in a circle around a central attracting mass, but as a matter of fact no such evolution can take place. In every instance of change from an incoherent to a coherent state, that is, from the condition of matter more or less scattered to matter forming a single aggregate, changes arising from the different circumstances of the different portions of aggregating matter must in every case occur. From the nebulous mass aggregating into a system of suns, down to the aggregation of the minutest drop or vesicle of fluid from vapour, or even in all probability down to the formation of molecules out of atoms (only no one knows what atoms or molecules in reality are), there must be varieties of condition in the forming mass causing differences of constitution in the mass formed ; at any rate I find myself unable, after long study, to think of any case in which an absolutely uniform process of aggregation takes place, or can ever be conceived to take place. Of compound processes, of course the illustrations are endless. Those we have already considered will serve as well as any others. Every process of formation, or of what Mr. Spencer here understands by the word evolution, must of necessity be varied in different parts of the forming aggregation by varieties in the conditions under which the formative process is applied to different parts of the aggregating material. Whether it be a stellar system forming out of star mist, or a solar system out of meteoric and cometic matter, or a single sun, or other celestial body out of multitudinous bodies before discrete—or, again, whether the aggregation is utterly unlike any studied by the astronomer, as the aggregation of a nation out of many races, or the formation of a society of any sort from scattered and before unassociated individuals, or the growth of an animal by the slow building together of material particles drawn from many sources, or the development of a race of animals by any process of evolution:—in every case, the various parts of the aggregate must differ *inter se*, because of the varying conditions under which they are severally formed.

5. *These secondary changes constitute a transformation of the homogeneous into the heterogeneous—a transformation which, like the first, is exhibited in the universe as a whole and in all* (*or nearly all*) *its details; in the aggregate of stars and nebulæ, in the planetary system; in the earth as an inorganic mass; in each organism, vegetal or animal* (*Von Baer's law otherwise expressed*)*; in the aggregate of organisms throughout geologic time; in the mind; in society; in all products of social activity.*

This amounts to the statement that processes of aggregation, affected as they are by varieties of condition, result in variety of structure. I venture to object to the statement that the homogeneous is transformed into the heterogeneous, because it implies that homogeneity can for a time (however brief) exist. This is contrary to experience. The process which actually takes place is a transformation from the less heterogeneous to the more heterogeneous, the strengthening or stronger marking of varieties of structure which began with the very beginning of the forming aggregation. Absolute uniformity never has existed or can exist in any part of the universe, large or great, any more than any absolute physical entity can exist which answers to the geometrical definition of a straight line. But the importance of the proposition before us is not modified by this necessary change. It remains true, that in the processes by which coherent aggregates of any sort whatever, and on whatever scale, are formed out of materials before separate and discrete, varieties which began at the very outset of the process become more and more marked as the process continues.

As an illustration, consider the formation of the solar system. We need not trouble ourselves to decide between the various theories which have been formed as to the actual way in which the solar system came into existence. Suffice it that whether the system was formed by the contraction of a mighty mass of nebulous matter, or by the aggregation of meteoric matter, the circumstances under which the planetary scheme came into existence were originally such that the growth of some—as of Jupiter and Saturn—was encouraged, while others, like Mars, Mercury, and the asteroids, acquired bulk and substance with difficulty. From the beginning the former planets were doubtless the larger ; and as time went on, their absolute, and probably their relative, superiority increased, until at length they came to occupy their present position, that is, to use the technical language which seems unfortunately coming into vogue in this subject, they became differentiated from the minor planets by their marked superiority in bulk and mass.

The law thus recognised prevails everywhere throughout the universe, in general as in detail. It is the physical form of the law, 'To him that hath shall be given, and from him that hath not shall be taken, even that which he seemeth to have.' Thus, in the case just considered, to the planets which had large bulk and mass, greater bulk and mass were given, while those which were smaller, lost, through the relative slowness of their growth, even that position which originally no doubt they seemed to have as members of a family of bodies not altogether unlike, and fell into a different and (so far as age is concerned) an inferior class. It is the same in all cases of systematic evolution. We are thus led to the law indicated in Mr. Spencer's next statement, the wording of which, like that of some which follows, is not altogether calculated to invite the attention of the unscientific reader, though the subject matters of Mr. Spencer's philosophy are such that all should understand its general purport.

6. *The process of integration, acting locally as well as generally, combines with the process of differentiation, to render the change described in* 5 *not simply one from homogeneity to heterogeneity, but from an indefinite homogeneity, to a definite heterogeneity; and this trait of increasing definiteness, which accompanies the trait of increasing heterogeneity, is, like it, exhibited in the totality of things, and in all its divisions and subdivisions, down to the minutest.*

In plain English this might be expressed somewhat as follows :—

When a whole is forming, the various parts not only differ because they are formed under different conditions, but because the formative process itself (acting on the various parts as well as on the forming mass or system as a whole) tends to produce different results in different parts. Thus not only is the product varied in character in its different parts, but the differences are definite. There is not an indefinite gradation from one form to another, but distinct steps of gradation so to speak,—and this is recognised throughout the universe, regarded as a whole, and in all its parts, and in all the parts of these parts, down to the minutest subdivisions.

For instance, in the solar system, the formation of a single system out of indefinitely distributed and moving matter, led to the formation of a central mass, and of bodies travelling around that mass, distinctly unlike it in character. Among the bodies thus travelling the formative process, acting diversely in different parts, and having varied quantities of matter and rates of motion to deal with, formed diverse families of bodies, the great planets in one group, the minor planets in another, the asteroids in a third—all definitely distinguished from each other. In biological evolution definite variations taking their origin at first in minute differences of condition, surroundings, and so forth, separate the various races, animal and vegetable: in each race the various genera become similarly distinct, while individual members of the same genus are also distinguished one from the other by peculiarities arising from the different constitutions under which each is formed. The same again is seen in the formation of distinct nations from among the various tribes and races of men, the characteristics which distinguish nation from nation becoming more and more marked as the nations gradually gather coherence and what may be called national individuality. Within one and the same nation class distinctions arise and become more and more marked with the progress of time. Within each class minor distinctions come into existence, and separate sub-class from sub-class more and more definitely. Still smaller subdivisions are formed, which in turn become more and more characteristically distinguished one from the other, till we reach the family, and finally the individual members of the family—the limits of subdivision in this direction. And so in every possible case, under all conceivable conditions, on the large scale as on the small, the law holds—aggregation is inevitably accompanied by the appearance of varieties of condition, quality, &c., in the parts of the aggregating mass, and, as aggregation proceeds, these varieties become more and more marked.

7. *Along with this redistribution of the matter composing any evolving aggregate there goes on a redistribution of the retained motion of its components in relation to one another; this also becomes, step by step, more definitely heterogeneous.*

As the aggregate is gradually formed, the motion of the matter forming the aggregate is gradually dissipated, as when masses which had been travelling freely around the sun or a planet are one by one brought to rest on the surface of the body whose mass they thus help to build up. But the motion is not altogether lost. It may, as in the case just considered, result in motion affecting the formed mass as a whole—the rotation in this case of a sun or a planet. It may result in systematic movement within the scheme or system thus formed, as in the movements of the planets within the solar system, or of satellites within a system circulating round a planet. But in the Spencerian philosophy motion is used in a wider sense. Thus the formation or evolution of a race of animals involves a dissipation of motion—the tendency to irregular changes being resolved into systematic variation—freedom to vary in any direction, merging gradually into the tendency to change only in specific directions and according to uniform law. So with other cases, even less like mere physical processes of aggregation, as we see in national, municipal, and social groupings. The law for each aggregate becomes more and more definite for each as time passes, precisely as the aggregates themselves become so.

We now come to statements belonging to the *à priori* aspect of the subject. It is evident that the only way in which we can conceive an utter absence of all tendency to the redistribution of matter and motion is by conceiving perfect uniformity throughout the entire universe : then and then only would all matter be related to all other matter in a manner absolutely indifferent, so that there would be no tendency either to aggregation or to any change in motions already existing, or to the state of absolute rest (if such were the condition of the primary and absolutely uniform universe thus conceived). Thus we have, next, the statements that,

8. *In the absence of a homogeneity that is infinite and absolute, the redistribution, of which evolution* [formation of

aggregates] *is one phase, is inevitable.* The causes which necessitate it are,—

9. *The instability of the homogeneous which is consequent upon the different exposures of the different parts of any limited aggregate to incident forces. The transformations hence resulting are complicated by,—*

10. *The multiplication of effects. Every mass and part of a mass on which a force falls, subdivides and differentiates that force, which thereupon proceeds to work a variety of changes; and each of these becomes the parent of similarly multiplying changes, the multiplication of them becoming greater in proportion as the aggregate becomes more heterogeneous.*

It will be observed that Mr. Spencer, in statement 8, recognises that only infinite and absolute uniformity could produce absolute stability. It appears to me that the same condition is required in order that evolution should be simple. For wherever, in the neighbourhood of any forming mass, or at whatever distance from it, there is a want of uniformity, the circumstances are not such that simple evolution can result; and we can imagine no circumstances in which, however remote might be the region where absolute uniformity ceased, the effect of such homogeneity would not in the long run be felt, resulting in what Mr. Spencer has described as compound evolution.

In statement 9 Mr. Spencer points out that uniformity in itself is essentially unstable. That which is uniform in structure inevitably tends to become diverse in structure when it is exposed to diverse conditions. The slightest breath of air will ripple the surface of level water, while powerless to affect the onward course of a wave. It is so throughout nature. Opposing forces may result in a condition of stable equilibrium; absolutely uniform conditions are, of their very nature, unstable.

In like manner, statement 10 needs little explanation and no proof. It is evident that in every process of evolution the various forces which produce various effects must be infinitely varied in their operation, according to the condition of the various parts of the aggregating whole. With every variation of their effects, the condition of that aggregating whole varies further. The variations thus arising may be cumulative in some parts or self-correcting in others, whence come into existence regions of greater variety and regions tending to such uniformity as results from counterpoised variations. Thus the aggregate becomes more and more varied in detail as well as in general—these sub-regions, so to speak, of uniformity dividing off regions of diversity. We may again use as an illustration the effect of winds upon the sea. The surface which had been uniform becomes uneven under the diverse action of the wind on various parts. Afterwards the wind, as it falls on the waves which traverse the water's surface, is modified in direction by their resistance and, being deflected in various ways, falls yet more diversely than before on the different parts of the water-surface : hence arises another kind of diversity, a minor order of varieties, which varieties in turn produce other and yet smaller forms of variety—the number of changes thus resulting being continually greater and greater as the surface becomes more and more disturbed.

We might find illustrations of this law in the star-depths, in the formation of a planetary system, in the shaping of such a world as our own earth. But we have illustrations more immediately interesting in relation to the general doctrine of evolution. What Darwin defines as the complex relations of animals and plants to each other in the struggle for existence affords an admirable illustration of the diversity of effects resulting from the inter-relation of varied action and varied condition in that which is acted upon. Consider, for instance, the following passage in the 'Origin of Species' :—'In several parts of the world insects determine the existence of cattle ; perhaps Paraguay offers the most curious instance of this ; for here neither cattle nor horses nor dogs have ever run wild, though they swarm northward and southward in a feral state ; and Azara and Reugger have shown that this is caused by the greater number in Paraguay of a certain fly, which lays its eggs in the navels of these animals when first born. The increase of these flies, numerous as they are, must be habitually checked by some means, probably by other parasite insects. Hence, if certain insectivorous birds do decrease in Paraguay, the parasite insects would probably increase, and this would lessen the number of the navel-frequenting flies—then the cattle and horses would become feral, and this would certainly greatly alter (as, indeed, I have observed in parts of South America) the vegetation ; this again would largely affect the insects ; and this, the insectivorous birds, and so onwards in ever-increasing circles of complexity. Not that under nature the relations will ever be as simple as this. Battle within battle must be continually recurring with varying success ; and yet, in the long-run, the forces are so nicely balanced that the face of nature remains for long periods of time uniform' [that is, with such uniformity as results from omnipresent variety] though assuredly 'the merest trifle would give the victory to one organic being over another.'

We are led directly to recognise among the causes of increasing variety :—

11. *Segregation, which is a process tending ever to separate unlike units, and to bring together like units—so serving continually to sharpen, or make definite, differentiation otherwise caused.*

As the result of these processes, a balancing of the forces at work arises from the matter worked on assuming those conditions which best favour their existence. The waves on our illustrative sea came to have just dimensions and just periods of oscillation on the greater scale, and they in turn are traversed by minor waves, and these by wavelets, and these in turn by ripples, harmonising with the winds and the variations of the winds which originally produced them. Thus, passing from the illustration to the processes of evolution illustrated.

12. *Equilibration is the final result of these transforma-*

tions which an evolving aggregate undergoes. The changes go on until there is reached an equilibrium between the forces which all parts of the aggregate are exposed to and the forces these parts oppose to them. Equilibration may pass through a transition stage of balanced motions (*as in a planetary system*), *or of balanced functions* (*as in a living body*), *on the way to ultimate equilibrium; but the state of rest in inorganic bodies, or death in organic bodies, is the necessary limit of the changes constituting evolution.*

This tendency to uniformity, really arising as a result of the constant subdivision and multiplication of diversities is seen on the largest scale (known to us) in the generally recognised tendency of this universe of ours to that condition of uniform temperature which would constitute its death, and on the smallest scale, in the natural death of animals and plants or of parts of these. Our sun is *alive* so long as, being of a higher temperature, he communicates heat to what lies around him; the stellar system is alive so long as some of its constituent parts are at higher levels of energy than the rest—just as a sea is active when the various parts of its surface are at different levels. The animal body is alive so long as the diverse energies of its various parts result in the processes of circulation and respiration. With uniformity resulting from the subdivision and distribution of energy comes death. But after death come processes akin to renewed life; though no longer the same life.

13. *Dissolution is the counter-change which sooner or later every evolved aggregate undergoes. Remaining exposed to surrounding forces that are unequilibrated, each aggregate is ever liable to be dissipated by the increase, gradual or sudden, of its contained motion; and its dissipation, quickly undergone by bodies lately animate, and slowly undergone by inanimate masses, remains to be undergone at an indefinitely remote period by each planetary and stellar mass, which since an indefinitely distant period in the past has been slowly cooling; the cycle of its transformation being complete.*

We can of course only infer from analogy that the heavenly bodies and systems of bodies after the equilibration of their energies with their surroundings—after, for instance, each sun has exhausted its superior heat, and therefore no longer ceases to part with heat to surrounding matter—will undergo a process of dissolution, thus completing the cycle of its transformations. It is so, we see, on the minor scale in every case with which we can deal. It is so with the individual members of animal and vegetable races, with families of animals and vegetables, with groups of these families, with nations, with social organisations. In every case we see how the life of each aggregate is limited in time, and tends to death, but how, also, after death the parts of the aggregate are dissolved, and become ready to take part in the formation of other aggregates. Hence—

14. *This rhythm of evolution and dissolution, completing itself during short periods in small aggregates, and in the vast aggregates distributed through space completing itself in periods which are immeasurable by human thought, is—so far as we can see, universal and eternal—each alternating phase of the process predominating now in this region of space and now in that, as local conditions determine.*

From the cases considered in the last paragraph, we can proceed with a certain degree of confidence to cases more extended, until we recognise in the solar system (for instance) the evidence of youth, and life, and old age, as stages of evolution,—though our processes and the range of our observation are too limited to enable us to judge (otherwise than from analogy) that after old age and death in these vast physical aggregations there comes a stage of dissolution completing the cycle of transformations. All we can say on that point is that, as in every case we can deal with to the end, we have found dissolution following the state of equilibrium which we call death in the case of the individual and compare to death in other cases, so also it is with those cases which (because of the limited range of our vision) we can only deal with in minute parts. We judge then that planets after this stage of death have a stage also of dissolution, though no physical experience enables us to say what that stage is like. We pass also to higher orders of being. We see suns of various ages throughout stellar space, and learn to recognise in their case also progression and evolution, up to and beyond the fulness and prime of stellar life. In their case also must come death, with equilibration between their energies and the receptive capacities of matter around them; and after this physical death must come, though in ways we cannot perceive, a process of dissolution, completing the cycle of transformations. So also with higher orders, with systems of suns, with systems of such suns, and so on, absolutely without end.

We come then to the final statements respecting the operations of nature and their significance. After what has been already explained, these need no words of mine to make them clearer. Nor could this paper be better closed than in the very words of this great teacher of our age :—

15. *All these phenomena, from their great features even to their minutest details, are necessary results of the persistence of force, under its forms of matter and motion. Given these as distributed through space, and their quantities being unchangeable either by increase or decrease, there inevitably result the continuous redistributions distinguishable as evolution and dissolution, as well as all those special traits above enumerated.*

16. *That which persists, unchanging in quantity, but ever changing in form under these sensible appearances which the universe presents to us, transcends human knowledge and conception,* IS AN UNKNOWN AND UNKNOWABLE POWER, WHICH WE ARE OBLIGED TO RECOGNISE AS WITHOUT LIMIT IN SPACE, AND WITHOUT BEGINNING OR END IN TIME.

A SURVEY OF THE NORTHERN HEAVENS.

THE ideas entertained by the non-scientific public respecting the extent and nature of the researches made by astronomers into the constitution of the heavens, are, for the most part, singularly inaccurate. Many suppose, for instance, that the astronomer knows the distances of at least those stars which are visible to the naked eye, and they hear with surprise that there are not six stars of the millions which astronomy really has to deal with whose distances can be regarded as even approximately determined. It is commonly supposed, again, that the whole surface of the celestial sphere has been so surveyed with powerful telescopes that astronomers know according to what laws the stars are distributed over the heavens. Few, indeed, even among those who may be called students of astronomy are aware how very far this idea is from the truth. We hear so often of the star-gaugings of the Herschels, and the results inferred from those star-gaugings are so confidently insisted upon in our text-books of astronomy, that when the actual extent of the Herschelian gauges is mentioned, the student is apt, in his sense of surprise and disappointment, to undervalue the labours of the Herschels as unduly as he had before exaggerated their extent and importance. Yet another mistake is commonly made. The range of the telescopes employed by astronomers is compared with the range of unaided vision, and men are apt to suppose that the astronomer obtains the same insight into the constitution of the heavens as though his powers of vision were correspondingly increased. The fact is forgotten that the telescope can only show the astronomer a minute portion of the heavens at a single view; that the information it is capable of supplying is in a sense piecemeal; and that the real lessons taught by the telescope, so far as the distribution of the heavenly bodies is concerned, can only be learned by combining together a number of large-scale views of separate portions of the heavens, into a single comparatively small-scale picture. It is not known that our materials for this work are for the most part incomplete, and that in those few instances where we have complete materials little has been done to utilise them.

I propose in this paper to consider, briefly, the extent of the researches hitherto made into the subject of the constitution of the star-depths; and then to describe some of the conclusions which seem deducible from an inquiry I have recently instituted, with the object of presenting in a single picture the results of one of the noblest series of labours yet undertaken by astronomers: Argelander's complete survey of the northern heavens with a telescope showing stars down to the tenth magnitude. Setting aside surveys limited to small regions of the heavens, it may be said that the only observational labours yet directed to the solution of the noblest physical problem man can study, are the star-gaugings of the Herschels. These star-gaugings constitute in reality but a minute proportion of the work achieved by these great astronomers. Yet, we may justly say, with Struve, that not one of the feats undertaken by the Herschels surpassed in daring that of attempting to gauge the star-depths with a telescope a foot and a half in aperture. If an astronomer devoted all his observing hours to such work, he would need, on a moderate computation, full eighty years to complete the survey of the heavens.[1] It is, therefore, with no thought of undervaluing the credit due to the Herschels for their star-gaugings, but solely because it is important that true ideas should be held on the subject, that I now refer to the relatively small extent of the heavens actually included in their survey.

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[1] 'Supposons que l'on puisse faire, pendant 100 nuits de l'année,

Sir William Herschel published, in 1785, the results of no less than 3,400 star-gauges. Each star-gauge gave the number of stars visible in the field of view of Herschel's telescope,—this field being equal in extent to almost exactly one-fourth of the apparent dimensions of the moon's disc; and in the course of these surveys—setting aside rough estimates on which Herschel himself laid no stress,—about 90,000 stars were counted.

At first sight we seem to have here a widely extended survey. The portion of the heavens actually gauged was equal to an extent which it would require 800 moons to cover; and thirty times as many stars as can be seen with the naked eye in the darkest and clearest night were actually counted. But when we leave the region of ordinary notions —whether as respects the moon's apparent size or the multitude of the stars—and enter into the real particulars, the startling nature of the disproportion between the extent of the heavens and the portion surveyed by Herschel is at once recognised. It is calculable that each field of view surveyed by Sir William Herschel amounted to but the 832,979th part of the celestial sphere. So that, in fact, he had gauged but about the 250th part of the area of the heavens. To obtain a clear idea of the minuteness of this proportion, suppose for a moment that the whole surface of the heavens is represented by an ordinary chess-board; then the combined extent of all the gauge-fields of Sir William Herschel would amount to barely the fourth part of one of the black or white squares of such a board.

chaque nuit 100 jauges, et il ne faudra pas moins de 83 ans pour le jaugeage du ciel entier. En effet, les 3,400 jauges de Herschel forment un de ses travaux les plus hardis.' Struve's *Études d'Astronomie stellaire*, note 74.

But even this illustration affords but an imperfect idea of the actual and admitted incompleteness of Herschel's table of star-gauges. One must turn to the table itself, to recognise at once the limited area of the field Herschel surveyed, and the difficult conditions under which such surveys are conducted by the astronomer. The great object Herschel had in view was, the determination of the relative number of stars visible in different directions; and it was obviously essential to his purpose that the conditions under which the gauges were made should be as nearly constant as possible. Yet we find that he notes opposite *hundreds* of his gauge-fields, that the observation was marred, either by haze, or moonlight, or twilight, or even by strong daylight, or by the light of the Aurora Borealis, or, lastly, by the low position of the gauged region.

As regards the number of stars counted by Sir William Herschel, we may recall to mind that, according to his computation, the Milky Way probably contains 18,000,000 stars visible in the 18-inch telescope he employed; while the indefatigable Struve considers that probably there are more than 20,000,000 such stars in the whole heavens. Thus we see that the 90,000 stars actually counted are relatively few, however large the number may seem by comparison with the number of stars visible to the unaided eye.

As respects Sir John Herschel's labours in the same field, it is only necessary to note that they were less extensive than his father's; the number of gauges amounting to 2,300, and the number of stars actually counted amounting to about 70,000.

In all, the star-gauges of the Herschels extended over the 150th part of the heavens, and included 160,000 stars actually counted. Let me repeat that I am far from desiring to undervalue these labours. The very fact that, among all the hundreds of astronomers who have studied the heavens with powerful telescopes, not one has yet rivalled either of the Herschels in this work of star-gauging, shows how highly the labours of those great astronomers should be esteemed. But I hold it to be essential that just ideas should be entertained on subjects of this sort. It is as unwise as it is wrong in principle to describe in misleading terms the labours of our great men, or to forget—as if due honour to their memory required such forgetfulness—to institute a searching comparison between the work they effected and what yet remains to be achieved. It is fortunate that men like the Herschels have ever been the last to encourage exaggerated estimates of their labours—insomuch that we find Sir William Herschel speaking of his star-gauging as intended only 'as an example to show the spirit of the method'—while Sir John Herschel again and again insisted on the necessity of fresh and more extended surveys than he and his father had been able to undertake.

The researches of the Herschels being the only observational labours directed by astronomers to the actual survey of the heavens with reference to the laws of stellar distribution, we have only to consider the inquiries directed to the analysis of those observations, in order to ascertain the sum and substance of the information actually obtained respecting the constitution or architecture of the sidereal system. When we inquire who are the astronomers who have attempted the task of educing from the labours of the Herschels their true value, we find that we have only to add the name of William Struve to the names of the Herschels themselves, in order to complete the list of such inquirers. Of course I am well aware that many others have discussed the works of the Herschels, and yet more have described the results to which the work has been held to point. But besides the elder Struve and the Herschels themselves, not one astronomer has subjected the observations of the latter to thorough investigation and analysis.

Now, there is one peculiarity in the several inquiries of the elder and younger Herschel, which deserves very careful consideration. Sir John Herschel unquestionably supposed, when discussing his own observations, that they confirmed the views of the elder Herschel—these views being those enunciated by Sir William Herschel in the famous paper of 1785, in which the star-gauges above referred to were first fully discussed. And, certainly, the statistical evidence obtained by Sir John Herschel corresponded very closely with that obtained by his father. But the remarkable point is, that *Sir William Herschel had virtually abandoned the views of* 1785, and that Sir John Herschel seems either not to have known, or to have forgotten, the circumstance! This must appear incredible, I am aware, to most of my readers, if not to all; more especially as all the text-books of astronomy agree in describing the views of Herschel in 1785 as those which he maintained to the close of his career; or rather, these books fail to mention that Sir William Herschel had in any respect modified his views. I feel that it will be a somewhat difficult task, under the circumstances, to establish the fact that the theory of 1785 was actually abandoned by the elder Herschel; but yet the evidence on the point appears to me so convincing, when rightly apprehended, that I shall at least claim the careful attention of the reader while I enunciate its leading features.

In the first place, in order to remove what must appear as a strong antecedent improbability in my view of the matter, let me remind the reader of the length of time which had elapsed between the death of the elder Herschel and the epoch when the younger Herschel undertook the supplementary series of star-gaugings in the southern heavens. The last of Sir William Herschel's papers on the general subject of the construction of the heavens appeared in the *Philosophical Transactions* for 1818, *thirty-three* years, let me note in passing, after the enunciation of the cloven disc theory of the sidereal system. Six years later, or in 1824, the elder Herschel died. Now the star-gaugings of the younger Herschel were not commenced until the year 1834, ten years after Sir William's death; nor have we any evidence that the younger Herschel's attention had been directed in a special manner to the subject of the star-gauges, either during the life of Sir William Herschel or afterwards during the progress of Sir John Herschel's observations in England. The eight years devoted by the younger Herschel to preparation for his observations at the Cape, were employed in the study of double stars, in the examination of nebulæ, and in other work of like nature, not in the comparatively rough work of star-gauging. At the Cape, even, star-gauging held a quite subordinate position in Herschel's labours. Here are the words in which he refers to the subject, in the noble work wherein he gives an account of his observations. After mentioning that his father's labours in 1785 had led to the theory that the sidereal system forms a stratum 'bifurcated or spread out into two sheets,' he proceeds :—'It may easily be supposed that the opportunity of carrying out this great induction by observations made with the same telescope, similarly used in that part of the heavens inaccessible to its author, was not neglected. So soon as a knowledge of the regions where nebulæ might more especially be expected had been acquired, so as not to hazard too much by continually interrupting the sweep for the purpose of gauging, a system of star-gauges was set on foot, at first somewhat irregularly, but after a very few sweeps, more systematically, so as to dot over the heavens, as it were, with a regular tesseration of gauged or counted fields, disposed at definite and equal intervals.' Then presently he mentions that 'many gauge points would needs escape observation, in consequence of the interference of the more important regular business of the sweep.' We find also that blanks were left where the heavens 'had been sufficiently swept over before the gauges commenced, or where, for other reasons, their registry was interrupted.'

So far as can be judged, then, the attention of Sir John Herschel was first specially directed to the subject of star-gauging at this epoch of 1834, ten years after his father's death, and half a century after the enunciation of the cloven disc theory of the sidereal system.[1]

For the sake of brevity, I will select the two most marked features of the theory of 1785; and I shall be able, I think, to show that Sir William Herschel adopted later altogether different opinions on both points.

It will not be questioned by those who are acquainted with the theory of 1785, or with what may be called the text-book theory of the sidereal system, that according to this theory the Milky Way is a congeries of stars, resembling our sun and the stars visible to the naked eye, and distributed in a similar manner throughout that portion of space which is occupied by our stellar system. Now, seventeen years later Herschel wrote as follows:—'Though our sun and all the stars we see may truly be said to be in the plane of the Milky Way, yet I am now convinced, by a long inspection and continued examination of it, that the Milky Way itself consists of stars very differently scattered from those which are immediately about us.'

Again, it will not be questioned that Sir William Herschel, when dealing with the star gauges of 1785, held richness of condensation to imply an enormous extension of the star-system in the direction where such richness was recognised.

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[1] If it should be objected, notwithstanding, that Sir John Herschel could hardly have forgotten any essential circumstances connected with a theory which his own father had enunciated, I would answer that, admitting those circumstances to have been fully known to him during his father's lifetime—which is not proved (and is even improbable in my judgment)—convincing evidence may yet be cited to show that Sir John Herschel might have forgotten them ten years after his father's death. For, in the Preface to the fifth edition of his *Outlines of Astronomy*, Sir John Herschel comments on certain researches of M. Reynaud as novelties, though, as he noted afterwards, he had himself formerly reasoned out the same subject to precisely similar conclusions—a fact which 'had completely escaped his recollection,' he says, 'when perusing the work of M. Reynaud.' Every student of science must, we conceive, have met with instances in his own experience rendering forgetfulness of this sort far from surprising.

This was, indeed, the essential principle of the method of star-gauging, the fundamental law on which that system was based. Yet, in 1802, Sir William Herschel thus absolutely negatives this rule of interpretation. After citing, as a convenient illustration of his new views, the two great clustering aggregations of the Milky Way between the stars Beta and Gamma, of the constellation Cygnus, he proceeds : 'These milky appearances deserve the name of clustering collections, as they are certainly brighter about the middle and fainter near their undefined borders. For in my sweeps of the heavens it has been fully ascertained that the brightness of the Milky Way arises only from stars ; and that their compression increases in proportion to the brightness of the Milky Way. We may, indeed, partly ascribe the increase both of brightness and of apparent compression to a greater depth of the space which contains these stars ; but this will equally tend to show their clustering condition ; for, since the increase of brightness is gradual, the space containing the clustering stars must tend to a spherical form if the gradual brightness of the stars is to be explained by the situation of the stars.' This passage, written as it is in Herschel's somewhat condensed style, may require paraphrasing ; but there can be no real question as to its meaning. What Herschel says amounts to this : the rich region with its bright centre and undefined borders is a clustering collection ; it may be that the richness of the central portion is due *altogether* to real richness of the cluster there ; but even admitting that the extra richness at the centre is partly due to greater range of the space containing the stars, yet still these stars must be held to form a cluster of a globular form ; for even if the gradual increase of brightness is to be thus explained by the situation of the stars, yet the way in which the increase takes place serves to prove that the space containing the clustering stars must tend to a spherical form. This is the obvious meaning of the passage ; and this interpretation of the appearances in Cygnus and elsewhere is wholly incompatible with the theory of 1785. According to this theory, the rich regions of the Milky Way corresponded to places where the stellar system extended to a vast distance, the richness in the telescopic field being due to the fact that the range of the instrument passed along an enormously extended array of stars reaching from the sun's neighbourhood to the extreme limits of the Milky Way. According to the views of 1802, the rich regions of the Milky Way correspond to regions within the star-system where real clusters exist—somewhat spherical in figure, perhaps, so that the line of sight through the middle of a cluster traverses a longer range than lines of sight towards the edge—but certainly true clusters, rich aggregations separated from surrounding space by a comparatively void region all around them. According to one view, the Milky Way might be compared to a fog surrounding the observer, the directions where the fog seems densest being those in which its extension from the observer is really greatest ; according to the other view, the Milky Way is to be compared to a bank of more or less closely set clouds, some of which—corresponding to the clustering aggregations in the Milky Way—can be distinctly recognised apart from the others. According to one theory, there is a continuous star-connection, so to speak, between the sun and the Milky Way—rich regions and poor alike ; according to the other, there is no such connection ; but the line of sight directed to the rich clustering aggregations of the Milky Way traverses, before reaching them, a relatively barren tract.

I have here taken the two most salient features of the theory of 1785, and have shown that these features are not only wanting in the views of 1802, but are replaced by features of a quite different character. I will now show that an astronomer who undoubtedly gave to the works of the elder Herschel a closer scrutiny than any other (not excepting Sir John Herschel) has cared to give, arrived at the conviction, not only that the theory of 1785 had been entirely abandoned by Sir William Herschel, but that the two main principles on which that theory had been based were given up by Herschel.

The elder Struve, in his admirable work, entitled *Études d'Astronomie stellaire*, gives an excellent digest of the papers of Sir William Herschel. It is well to note the circumstances under which Struve studied Herschel's papers, for otherwise the full importance of his conclusions will not be recognised. He had already given the papers one reading, and had failed to notice how the views of the elder Herschel changed between 1785 and 1802.[1]

This is not surprising, when we remember that Herschel's papers are written in a somewhat terse style, and that when exhibiting new views he does not point out where and how these differ from opinions he may have before expressed. When we consider that the papers written by Herschel range over nearly half a century, it will be understood that they do not form a perfectly congruous series; and at the same time we can understand that the progression and change of ideas would not, at a first reading, be rightly apprehended. I certainly found this to be the case in my own first study of Herschel's papers; insomuch that I have written many passages in books and essays as though the views I advocated were opposed to his, when in reality they were accordant with his later opinions. And I believe that no one, unadvised of Herschel's change of view, and studying his papers from beginning to end with an ordinary degree of attention, would arrive at any other conclusion than that to which Struve came in the first instance, and that which I conceive Sir John Herschel entertained to the end of his life. But in 1830 (let the date be noted in connection with what has been remarked respecting the younger Herschel's star-gauging) Struve received from Sir John Herschel a valuable present—a complete copy of the elder Herschel's papers, the author's original manuscript enriched by his corrections and by several manuscript notes. It was this noble gift that led Struve to the thorough investigation (*à une étude assidue*) of the works of the elder Herschel.

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[1] This is to be inferred from the following passage. After speaking of his own earlier researches, he notes that the facts he is now about to present must be regarded as 'une discussion ultérieure à laquelle je me suis engagé, surtout par une nouvelle étude des mémoires de Sir W. Herschel, faite depuis, et dont les résultats ont été exposés dans la partie historique de ce rapport. En effet, je me suis convaincu que mes idées actuelles sur la voie lactée ne sont en opposition qu'avec le système de Herschel de 1785, mais extrêmement conformes aux vues postérieures de ce grand astronome.'

Struve shows clearly that the theory of 1785 is based on two hypotheses, the failure of either of which (by Herschel's own admission) would render the theory inadmissible. The first is the hypothesis of a generally uniform distribution of stars throughout the sidereal system ; the second is the hypothesis that the gauging telescope was capable of penetrating to the limits of the sidereal system in all directions. Then Struve quotes passage after passage belonging to the papers of 1802 and later years, in which these hypotheses are definitively abandoned by Sir William Herschel; and he arrives at this noteworthy conclusion, that the theory of 1785, respecting the constitution of the Milky Way, is completely negatived by the later researches of its author, and that Herschel himself had completely abandoned it.[1]

Yet Struve very properly combined the observations of 1785 with his own researches. Clearly recognising the limited range of those observations and the necessity for further survey of the heavens, he yet took advantage of the numerical statistics they afforded to supplement the relations he was the first to discuss ; those, namely, which are to be recognised in the statistical distribution of stars of the higher or brighter orders of magnitude.[2]

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[1] Struve's words are as follows : 'Nous parvenons donc au résultat —peut-être inattendu mais incontestable—que le système de Herschel, énoncé en 1785, sur l'arrangement de la voie lactée, s'écroule de toutes parts, d'après les recherches ultérieures de l'auteur ; et que Herschel lui-même l'a entièrement abandonné.'

[2] There is some confusion of expression as to the different orders of star magnitude. Some astronomers speak of the fainter stars as belonging to the higher orders, because larger numbers are employed

Struve's researches were carried out with the express object of determining whether the arrangement of stars of the brighter orders of magnitude affords any evidence respecting the constitution of the sidereal system. Piazzi had noticed that even among the stars visible to the naked eye there is a tendency to aggregation towards the Milky Way: a phenomena clearly irreconcileable with the theory of 1785. For, according to this theory, the stars are spread with a certain general uniformity throughout the stellar system, and the richness of the Milky Way is due to the greater extension of the system in the direction of a certain stratum. Within this imagined stratum, a cross-section of which illustrates Herschel's paper, a sphere enclosing all the stars visible to the unaided eye would be completely enclosed, or rather, immersed. Herschel tells us, indeed, the proportion which he conceived to exist between the dimensions of his section and the extent of his sphere of naked-eye stars, or *lucid* stars, as they are called for distinction. The section is 10½ inches long and 2½ inches wide, and he remarks that this section 'is drawn upon a scale where the distance of Sirius is no more than the 80th part of an inch; so that probably all the stars, which in the finest nights we are able to distinguish with the naked eye, may be comprehended within a sphere drawn round the large star near the middle, representing our situation in the nebulæ of less than half a quarter of an inch in radius;' that is a sphere separated by a distance of more than nine times its own radius from the flattened sides of the imagined stratum, and by more than

to indicate those orders. Others—and, on the whole, their plan seems preferable—speak of the fainter stars as belonging to the lower orders of magnitude. So far as analogous numerical instances are in question, both methods can be justified; for the mathematician speaks of equations of high order when the number expressing the order is large; and, on the other hand, we commonly speak of persons or objects as low down in a list when the number expressing their place is large. Since the fainter stars are inferior in the primary quality of a star—that is, lustre—it would seem that those orders which are expressed by the larger number, being thus inferior, should be called the lower orders.

forty times that distance from the rim of the stratum. It is quite obvious that the construction of this small sphere could be in no way associated with the construction of the stratum, if the theory were just, any more than the disposition of the water vesicles within a space a foot in diameter in the heart of a cloud could be associated with the figure of the cloud.

Struve's first inquiries were applied to stars down to the seventh magnitude inclusive. He showed that these stars are not spread in a uniform manner over the heavens, but are more densely aggregated upon and near the zone of the Milky Way. The method of research he employed was perhaps not altogether satisfactory, since he merely counted the number of stars included severally, according to his catalogue, in the twenty-four equal spaces into which the hour-meridians [1] divide a series of zones parallel to the celestial equator. The spaces were too large to afford more than a rough estimate of the laws of distribution. But still there was so obvious an increase of richness when the Milky Way was approached, so marked a poverty near the poles of the galaxy, that Struve was able to announce with confidence that the theory of 1785 was unsound.

When a careful re-examination of Sir W. Herschel's papers had shown him that Herschel had himself abandoned the theory of 1785, Struve undertook a new series of researches. He now employed a much larger number of stars than he had before dealt with. M. Weisse had reduced and arranged the stars observed by Lalande, Bessel, and Argelander, so far as a zone of the heavens, thirty degrees wide, was concerned. This zone, ranging from 15 degrees north to 15 degrees south of the equator, contained no less than 31,085 stars, from the first to the ninth magnitude, inclusive; of these 1664 belonged to the first six orders of magnitude, 2,500 were of the seventh magnitude, 8,183 of the eighth, and 19,738 of the ninth. But all the stars of these orders, and belonging to the zone, were not included in Weisse's catalogue. By an elaborate process of examination, during which he showed a singular mastery of the laws of averages, Struve estimated how many stars of the different orders had been missed in different parts of the zone; and in this way he increased the number of stars virtually available for his purpose from 31,085 to 52,199, a number fairly comparable, it will be noticed, with the 70,000 stars counted by Sir John Herschel and the 90,000 stars counted by his father.

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[1] Circles through the poles of the heavens, dividing the celestial uator into twenty-four equal parts.

Struve conceived the zone of stars to be divided into twenty-four equal parts by hour-circles, that is, by division-lines square to the equator, or to the length of the zone. Then, as in his former researches, he compared together the numbers of stars in these twenty-four equal divisions. He again found that the divisions crossed by the Milky Way and those lying near to the Milky Way, are richer in stars than the rest. The evidence was of the most decisive nature, and Struve rendered it more convincing by the way in which he repeated his tests. Thus, whether the twenty-four divisions were separately considered, or whether he took them in opposite pairs, or whether he took them in pairs lying at equal distances on either side of the points where the Milky Way crosses the equator, or whether, lastly, he compared what may be called the Milky Way divisions with the remainder: in all cases he found the evidence decisively in favour of a condensation of stars, of the leading orders of magnitude, towards the galactic zone. For the sake of brevity, I quote only the last of the four above-mentioned tests. The Milky Way crosses the equator near the hour-divisions 6 and 18; more exactly, the main line of the Milky Way crosses the equator at the divisions marked 6h. 40m., and 18h. 40m.; so that the divisions corresponding to the hours 5, 6, 7, 8, on one side, and 17, 18, 19, 20 on the other, are those which may be called the galactic divisions. Now, the first set of four divisions contained 13,593 stars, the other set 10,657 stars, giving an average of 12,125 stars for each set of four divisions, and 3,031 stars for each division. The remaining sixteen divisions contained 27,946 stars, or an average of 1,747 stars for each. There can be no question, then, that a tendency to aggregation along the galactic zone exists among the stars of the first nine orders of magnitude.

Struve extended his inquiry, so as to include the consideration of the distribution of the 52,000 stars in space. This he did by conceiving his zone of stars to be converted into a flat disc in the plane of the equator, and the stars of the several orders spread uniformly as respects radial directions—that is, in directions tending from the sun, the imagined centre of the system of stars. This part of Struve's work seems open to serious objection. The general lesson deduced from the former part of the work was good, so far as it went; but, in the use now made of the results, Struve certainly pushed them farther than was permissible. What he had learned was, that certain divisions of the heavens, 30 degrees long and 15 degrees wide, were severally more or less rich in stars. It seems sufficiently hazardous to lose consideration of the length of these divisions, and to suppose that their several degrees of richness adequately represent the richness of the heavens along their medial line. But to proceed farther, and to assume that outwards from the eye towards that medial line there was a general uniformity of distribution, was perfectly incompatible with the lesson taught by the varying richness of the several original divisions. That lesson clearly was, that *any* assumption of uniformity is inadmissible until evidence has been obtained which renders it to some degree probable. Struve had not a particle of evidence in favour of the assumption he made. That assumption amounted, in fact, to the hypothesis that there is a general approach to uniformity of *magnitude* among the stars—so that Struve, who had clearly disproved the old hypothesis of a general uniformity of *distribution*, was now adopting an hypothesis at least as little likely to approximate to the truth.

Let me now be permitted to present my own researches directed to the solution of the same problem. My inquiries have been in a sense parallel to those made by Struve; but they have been conducted on a different plan. It will have been noticed that Struve dealt throughout with numerical statistics, and that he discussed these according to the law of averages, regarding minor details as probably unimportant. I have followed always the method of equal surface (*isographic*) charting, whereby numerical statistics are in effect presented to the eye; the method having this further advantage, that it exhibits (or may be employed to exhibit) minor details as well as general relations.

The value of this method has been impressed upon me during a long, and, I believe, quite exceptional course of star-charting. In 1866, I drew my Gnomonic Chart, showing all the stars down to the fifth magnitude inclusive. In drawing the maps of my 'Constellation Seasons,' I had occasion to picture twelve several times the relations among the stars visible in England, down to the fourth magnitude inclusive. And afterwards the same series of maps, but showing only the stars down to the third magnitude, was constructed for my 'Half Hours with the Stars.' It was the recognition of peculiarities of arrangement even among the relatively small number of stars included in these several series, that led to the enunciation of my earliest ideas respecting the laws according to which the lucid stars are distributed.

Then followed the construction of my large Star Atlas, in which stars down to the sixth magnitude are included; in other words, about 5,900 stars instead of the 1,500 of the Gnomonic Atlas. There was, also, this peculiarity about the large atlas, to render the progress of the work more instructive than it would otherwise have been: for the first time since star-charting began, the stars visible to the naked eye were all pictured without appreciable distortion and on a systematic plan, bringing into recognition the actually existent but hitherto unsuspected laws according to which those stars are distributed. It was natural that I should take the earliest opportunity to make an actually isographic chart of these stars—that is, that I should present these stars in a chart in which equal spaces on the globe should be represented by equal spaces on the chart. The results of this work, and of the application of a method of research, which I called the 'balance and scissors' method, were described in a paper which appeared in 'Fraser's Magazine' several years ago.[1]

But I have long been anxious to apply this method to a far larger number of stars than the 5,900 shown in my atlas. Large as this number may appear, yet by comparison with the numbers dealt with by Struve, and still more with the numbers counted in the Herschelian gauges, it appears insignificant. And although the conclusions which I had actually educed from the distribution of these stars are not in reality open to question—since I have been able to show that the laws of probability will not permit us to regard the observed relations as accidental, yet they may not be judged by some so convincingly established, as relations founded on an overwhelming array of instances.

Accordingly, I had for some time desired to chart on a single isographic projection, all the stars—324,000 in number—included in Argelander's splendid series of charts of the northern heavens. These charts show all the stars of the first nine orders and of the order intermediate to the ninth and tenth over the whole of the northern hemisphere and within two degrees south of the equator. Each chart forms a sheet 20 by 30 inches in extent; and my purpose was to combine all these charts into one circular chart on a single sheet.

The first point to be attended to was, that the maps should only be commenced when it was possible to carry on the work without interruption to its close. To have drawn a portion of the chart, and then to have left it even for a few days, while the work was in progress, was to risk a change in the style of mapping, which would have rendered the map almost valueless so far as my purpose was concerned.

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[1] These results were first published in a paper read before the British Association, during their meeting at Liverpool in 1873. The isographic chart illustrates the second edition of my *Other Worlds*, where also the results I deduce from the chart are described and analysed.

But to secure time for the charting of 324,000 stars—each star to be carefully copied in from a large original—was not an altogether easy matter. On a moderate computation, a minute would, on the average, be required for marking in ten stars; and this gave 540 hours for the star-charting alone; that is, at the rate of 60 working hours per week, 9 weeks would be required for the work. It was with difficulty I could secure 7 clear weeks. During this interval, however, I succeeded in completing the chart.

Before the charting was commenced, the projection was laid down in pencil, to every degree, that is, there were 360 radial lines and 92 concentric circles. Near the centre some of the radial lines were omitted. In all, the pencilling divided the chart into 26,400 spaces, corresponding to the spaces in Argelander's charts. Thus the average number of stars in each space was but eighteen; and it was easy to make each space a sufficiently accurate miniature of the corresponding space in the large original. When all the 26,400 spaces were thus filled in, there appeared at a single view all the stars which we should see in the northern heavens if our powers of vision were increased a hundredfold. Twice as many stars as both the Herschels counted, 10 times as many as Struve discussed in his 'Études,' and about 130 times as many as can be seen with the naked eye over the whole of the vault of heaven on the darkest and clearest night, were now for the first time (thanks to the seven years' labour of Argelander) presented to the eye at a single view; and it remained only to study the relations which could be recognised among these thirty myriads of stars.

These relations are full, I conceive, of significance.

In the first place, the aggregation of stars along the galactic zone, which Struve had inferred from numerical statistics, was amply confirmed. But more than this. I had long since expressed my belief that it is not the galactic zone, but the galaxy itself, towards which the stars are aggregated. The distinction is of importance. When astronomers speak of the condensation of stars along the galactic zone, they imply that there really is a distinct zone of the heavens formed by the Milky Way, and that the stars are spread with a gradually increasing degree of richness the nearer that zone is approached. Now, it has been known for many years that so far as the visible Milky Way is concerned, no such *zone* exists. Across the widest part of the Milky Way there is a broad gap completely marring the completeness of the so-called zone. Then, in another region, the Milky Way consists of a multitude of interlacing branches, intermixed with lakes and islands of light and darkness, forming a galactic region complex beyond expression. These features belong to the southern half of the Milky Way; and we owe our knowledge respecting them to those researches of Sir John Herschel which were in progress when Struve wrote his 'Études.' But even when speaking of the northern portion Struve had been constrained to admit that a fresh survey was needed, before the Milky Way could be regarded as a zone, simply divided into two, along one part of its extent. And, accordingly, we find that when carefully studied, the northern half of the Milky Way presents a complexity of structure which the accepted description and pictures are far from conveying. Wollaston has referred to branchlike projections both on the northern and southern side of the zone, in the constellation Perseus, and also in the divided portion of the Milky Way from the Swan to the equator. Sir John Herschel described how a branch extends from Cepheus towards the North Pole; and another from Perseus towards the Pleiades; neither of these branches appearing in Wollaston's description. A single dark space is seen in the constellation of the Swan by some observers; but others recognise two dark spaces. Some have regarded the second branch of the Milky Way from the Swan as continuous; but Sir John Herschel has pointed out that the branch does not extend beyond the equator southwards. In fine, no two pictures or descriptions of the Northern Milky Way agree even as respects main features, and far less in minor details. The cause of this is undoubtedly the real complexity of the galaxy, a complexity well described by the late Professor Nichol, in the following noteworthy passage: 'It is, indeed, only to the most careless glance, or when viewed through an atmosphere of imperfect transparency, that the Milky Way seems a continuous zone. Let the naked eye rest thoughtfully on any part of it; and, if circumstances be favourable, it will stand out rather as an accumulation of patches and streams of light of every conceivable variety of form and brightness; now side by side, now heaped on each other, again spanning across dark spaces, intertwining and forming a most curious and complex network; and at other times darting off into the neighbouring skies in branches of capricious length and shape which gradually thin away and disappear.

Now, in my chart all the chief features of the Milky Way are actually shown by the mere marking in of the stars belonging to Argelander's chart. In other words, *these* stars so congregate on the galaxy as to form a picture of it. I had thought of marking, on some photographs of the chart, the outline of the Milky Way; but this was unnecessary, simply because the stars in the chart showed the Milky Way precisely as in a stippled engraving. Wollaston's three projecting branches in Perseus, and Sir John Herschel's branch towards the Pleiades, are plainly shown, so also the projection from Cepheus towards the Pole, the great rich region in Cygnus; in fine, all the features commonly described and pictured. Not, however, with the uniform tint and the well-defined outlines which are shown in maps; but (precisely as Professor Nichol described the Milky Way) with a wonderful complexity of interior structure, and the edges thinning away and gradually disappearing.

When we remember that Sir William Herschel during his sweeps (as distinguished from his gauges) established the general principle, that where the Milky Way is brightest to the eye there the stars shown in his powerful telescope were most numerous, we recognise the significance of these circumstances. We see that since, where the Milky Way is brightest, stars of all orders down to the faintest Herschel could see, *as well as* stars of the first nine or ten orders of magnitude, are most numerous, it follows that richness in stars of the last-named orders is a phenomenon associated with richness in all orders of stars down to the faintest seen in the 20-feet reflector. *This can only be interpreted in one way.* Let us take a parallel instance. Conceive myriads of birds visible over the sky, their real size and nature unknown, but two orders of apparent size recognisable. If nothing further were known, the inference that all the birds were alike in size, but that those which seemed smallest were flying at a higher level than the other set, would be as probable as the inference that two kinds of birds were flying together at one level (of course, all questions of the ways of birds is here set aside, for the sake of our illustration). But if there were certain parts of the heavens where the birds were much more densely crowded than elsewhere, and if it was noticed that in these regions without exception birds of *both* sets were more numerous than elsewhere, no other conclusion could be arrived at than that the two sets were intermingled, and therefore really unlike in size. For to suppose otherwise would be to imagine that two sets of birds flying at very different levels, so arranged themselves in their respective strata that whenever the line of sight from the observer on earth passed through a dense cluster belonging to one set, it also passed through a dense cluster, much more distant, belonging to the other set. This, of course, would be too unlikely for belief, more especially if the richly clustered regions were very numerous and complex. Precisely the same reasoning applies to the observed arrangement of stars. We cannot suppose that clustering aggregations of stars which are relatively near to us, lie always in the same direction as corresponding clusters of stars which seem faint through excessive distance. We must, therefore, conclude that these seemingly associated groups of faint and (relatively) bright[1] stars, are really intermingled; *in other words, that the fainter stars in these groups owe their faintness to real relative minuteness.*

Another circumstance is to be noticed in the chart. The projections of the Milky Way can be traced farther from the main stream than when the galaxy is studied with the unaided eye. A clearer idea is thus afforded of the real nature of this marvellously complex system of stars. The idea which I advocated some time since, that the Milky Way is a spiral, is confirmed; but the spiral can no longer be regarded as consisting of one or two whorls lying nearly in the same plane. It appears rather as a highly complex kind of helix, the spirals of which are not merely numerous, but themselves formed of numerous convolutions.

It is noteworthy, though I am not at present prepared to insist on the circumstance, that these branching extensions of the Milky Way, when thus traced beyond the limits recognisable by ordinary vision, seem to unite with the branches of the great nebular regions. It is well known that these nebular regions lie apart from the Milky Way. In the 'Proceedings of the Astronomical Society for 1868–69, there will be found three engravings, in each of which I have given a complete view of these nebular regions, as seen in different aspects. The outlying branches are very marked in character, and certainly agree in position in a very singular manner with regions towards which the branches of the Milky Way seem to point. It would almost seem as though those mysterious nebular regions are merely distant extensions of the projecting branches of the galaxy.[2]

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[1] The faintest stars in Argelander's chart bear about the same relation to the faintest shown in Herschel's 20-feet reflector that a star of the first magnitude bears to the faintest which the naked eye can detect.

[2] This is a matter for further inquiry. I propose to apply a method of research to the subject which can hardly fail to educe the true

One of the features I will touch upon very briefly, partly because this paper has already exceeded the limits I had proposed to myself when I commenced it, and partly because, without illustrations, the real nature of the peculiarity could hardly be adequately exhibited. I refer to the arrangements of the stars in streams and sprays. Here I do not speak of star-streams, such as those recognised by the ancients in the constellations Aquarius, Hydra, and Eridanus, but to minute wisps (as it were) of stars, whose figure seems associated in a significant manner with the regions in which they appear. In some parts of the charts this arrangement is so peculiar and complicated as to give the portion of the heavens represented an appearance resembling the twig-work of a large tree, as seen projected against a winter-sky.

But the teachings of the chart are not yet exhausted. Peculiarities of structure present themselves, which suggest unexpected relations among the orbs which people space. The nature of some of these peculiarities can only be understood by a reference to the chart itself; and respecting others, I forbear to speak at present, in order that at another time I may discuss them more fully than space will here permit. Perhaps the most important result of the construction of the chart is the information afforded as to the regions of the heavens which are most likely to reward the star-gauger. The chart shows what regions of the heavens should be selected for this kind of survey. I believe that telescopists cannot more effectually advance our knowledge of the structure of the heavens than by joining in this much-needed work.

meaning of the observed relations. It is not impossible that in a short time I may be able to publish the result of my inquiries.

STAR UNTO STAR.

When nearly twenty years ago, Drs. Huggins and Miller published the first results obtained from the spectroscopic study of stars, few could have supposed that a line of research so difficult and delicate would lead to the bold and startling views of the star-depths which now seem opening out before us. Still less would it have been thought that the method of research would be so modified that the observations belonging to it could be pursued without the direct personal study of the stellar spectra which have been found so difficult, and even (where exact researches were in question) so painful. In 1864 the observer who wished to determine whether a special substance existed in the vaporous atmosphere of a star, had to compare the spectrum of the star directly with the spectrum of the substance. In other words he had first to turn his telescope upon the star with such precision that the image of the star should fall on the fine slit of the spectroscope (and be kept there by clock motion driving the telescope, throughout the operation), and the light of the star being then sifted out by the action of the prisms in the spectroscope, so as to form a rainbow-tinted streak or spectrum crossed by dark lines where certain tints are missing (on account of special absorptive action in the vaporous atmosphere of the star), the observer had to bring into the same field of view, and into precisely corresponding position, by the action of the same spectroscope, the bright line spectrum of whatever substance he wished to deal with. If the bright lines forming the spectrum of magnesium, or sodium, or calcium, or the like, were found to correspond exactly with dark lines or missing tints in the spectrum of the star, then the observer would know that the particular substance giving those bright lines (or, more correctly, shining with those tints) existed in the atmosphere of the star. But he might very well be in doubt as to the precise accuracy of the coincidences (on which everything depends), or he might not be able to perceive clearly, yet might suspect the existence of one or other of the dark lines necessary to complete the evidence. To make sure he must cause the electric spark producing the spectrum of the substance he is dealing with to flash again and again out of the darkness, wearying the eye by the constant alternation of darkness with bright light. Not a few minutes, but many hours, on even several observing nights, would be required for each observation of the sort; and later, some other observer, with different visual powers, or with instruments of greater or less precision, might throw doubt on the accuracy of the observation, and the whole work might have to be repeated.

Now, all this is changed. A photographic record of the spectrum is taken (hitherto only of the blue, violet, and ultra-violet part, but before long the whole visible spectrum, and parts invisible beyond the red and violet, will doubtless be photographed), and either at the same time or under precisely the same optical conditions, a photograph of the sun's spectrum (not taken directly from the sun, but either from the twilight sky or from a planet like Venus which reflects pure sunlight), and then the known dark lines in the solar spectrum can be compared directly with the dark lines in the spectrum of the star. If doubt be afterwards thrown on the result, the slips with the recorded photographic spectra are always available for comparison. And thus star after star can be added to the list of those whose light-record of their vaporous structure has been obtained. Fainter and fainter stars can be dealt with as the delicacy of sensitive plates is increased, or as the accuracy of the clock-driving of telescopes is increased, until the photographic plate may be exposed during the whole of any clear night to receive the light impressions from a star. Already Dr. Draper has obtained records of the spectra of stars of the tenth magnitude—that is, far beyond the range of ordinary vision—though as yet such records of faint stars have not been available for the kind of research we are considering. In fact they have only been received accidentally, so to speak, when search was being made for something entirely different.

We are not, however, here concerned to consider at any length the methods employed. It is interesting, and will appear more so as we proceed, to note how widely the research we are considering is likely to be extended in the future. But at present we propose chiefly to discuss the most remarkable result which has rewarded the method of spectroscopic inquiry into the stars, whether by ordinary vision or by the use of photographic appliances.

The result to which we refer is the marshalling of the stars into orders, different in colour, which spectroscopic analysis shows to be due to difference in present physical constitution, which again analogical reasoning shows to be due to difference in age.

Take first the bluish-white stars of which Sirius, Vega, Altair, and others are typical.

In the first place, we note that the only star of this order whose distance has been even roughly determined (Alpha Centauri in the southern hemisphere is a yellowish-white star) is demonstrably a much larger orb than our own sun, if the quantity of light which a sun emits is any indication of size. Sirius is so remote that the motion of the earth in her vast orbit, 185 million miles in diameter, scarcely at all affects the apparent position of that brilliant star. Very exact and careful study of the star indicates apparent motion due to the earth's real motion in a tiny ellipse, the larger axis of which is roughly about the 4,000th part of the moon's apparent diameter—the nature of the observation being such that this larger axis *may* be as much as the 3,000th or as little as the 5,000th part of the moon's apparent diameter, or even lie outside those limits. Taking the mean of the best measurements, a distance is inferred so great that our sun's light, were he placed at that distance, would be reduced to about the 50th part of the apparent lustre of a first-magnitude star, or, roughly, to about the 200th part of the lustre of Sirius. Hence it would follow that if an average square mile of the surface of Sirius emits as much light as an average square mile of the sun's surface, the surface of Sirius must be 200 times as large as the surface of our sun. If so, the diameter of Sirius would be about 14 times the diameter of the sun (for 14 times 14 are 196), and his volume about 2,800 times, or in round numbers 3,000 times the volume of the sun. We can hardly suppose that his volume, or probably his mass, is less than a thousand times larger than the sun's.

Of other stars of the bluish-white order we know less, with precision, but we do know so much as this, that all the brighter ones *must* be, and therefore even the fainter ones *probably* are, very much larger than the sun. For though the actual distance of Vega and Altair, for example, cannot be determined, it is because they are so far away that attempts at measurement fail. If either of them were as near as Sirius, its distance would be as readily determinable. But the measures which, applied to Sirius, give a recognisable result, fail utterly when applied to Vega and Altair. It is true, results are published in our books of astronomy which if accepted would indicate a measured distance in the case of Vega, but it is utterly untrustworthy. Vega and Altair lie beyond the range of the best methods of measurement yet invented. But noting that their lustre still exceeds many times that which the sun would have if removed to the distance of Sirius, we infer safely that the lustre of those two bluish-white stars exceeds in yet greater degree that which our sun would have if removed to their distance : in what precise degree we cannot determine, but we may confidently say that these stars are very much larger than our own sun. The same argument applies to all the brighter stars of the bluish-white kind. And having thus inferred that so many stars of this colour exceed our sun in size, it is a highly probable inference that all do (the fainter being simply much farther away), *if* it shall appear that all the stars of this kind possess certain physical characteristics which stars of other colours do not. For if it is a fair inference that because all bluish-white stars yet examined possess such characteristics, so will others of the same colour which may hereafter be examined; and, again, that because no other stars have yet been found to possess these characteristics but stars of a bluish-white colour, therefore others which may hereafter be found to possess them will also be of this colour, it is clearly as fair an inference to assume that the great size characterising all the stars of this kind yet tested or testable in this respect is a characteristic also of the class.

Now it appears from direct spectroscopic study of these stars, as well as from their spectra, that they differ in physical structure in marked manner and degree from our sun. The lines which indicate the presence of relatively cool hydrogen —hydrogen exerting an absorptive action on the light from the central glowing mass—in these stars are always much stronger and broader than in the spectrum of our sun. I do not dwell here on a question which has arisen as to a certain line which appears to be common both to calcium and to hydrogen, and has therefore given rise to certain discussions (running, in my opinion, far in advance of the evidence) as to the identity of some element common to both calcium and hydrogen, which of course, according to that view, would neither of them be elements. I prefer now to consider only the broad lines of distinction between the various orders of stars, and not to discuss *minutiæ* which may hereafter very probably be shown to be altogether without significance.[1]

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[1] Just as Professor Young, by using spectroscopes of great dispersive power and showing lines to be diverse which with inferior instru-

Now the great breadth and strength of the hydrogen lines in these monstrous suns (suns exceeding our sun much as our sun exceeds Jupiter and Saturn, and as these planets exceed our earth, Venus, and Mars) may be taken safely enough to indicate the existence of much deeper and denser atmospheres of relatively cool hydrogen around those suns than exist around our own. Yet the intense brightness and whiteness of those suns serve to show that such deep envelopes of relatively cool hydrogen are by no means due to the longer continuance of a process of cooling. On the contrary, it seems clear that it is the greater intensity of the radiation of those parts of the stars' light which form the continuous background of the spectrum, and not the greater intensity of the absorptive action of the hydrogen, which really occasions those lines to appear broad and dark. The hydrogen itself, which, owing to the great lightness of this element under the same conditions of temperature and pressure, extends high above the other gaseous envelopes, forming the outer parts of these intensely bright white stars, is no doubt itself intensely hot. Most probably it is far hotter than those hydrogen layers which cause the finer absorptive lines of hydrogen in the spectrum of our own sun and his fellows. But so much more intense is the light radiated from the glowing mass within (mostly from glowing gas at great pressure, I think) that the absorptive lines of hydrogen appear by contrast very broad and very strong.

On this view we may fairly assume that the darkness of the hydrogen lines is a characteristic of stars at a much higher temperature than our sun and suns of the same class. And finding this characteristic associated with some stars which are certainly of enormous size, and with other stars which *may* be thus exceptionally large, we are led to infer that this association is not accidental—that *all* stars having

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ments had seemed identical, has entirely destroyed the imagined validity of evidence on which certain very bold assumptions as to the elementary constitution of matter had been based.

these very strong hydrogen lines are very much larger than own own sun.

Whether we can accept this inference or not will depend very much on whether we can regard the youth of a sun as in any way correlated with the sun's size. The reasoning which I have applied to planets—the justice of which reasoning seems confirmed by the accordance of the results to which it leads with observed facts—may be applied also to the stars. I have shown that if two planets of different size are at any given epoch in the same stage of planetary life—that is, at the same temperature—the smaller will presently pass into a more advanced stage than the larger will have attained to, because it will part with its heat at a relatively greater rate. Supposing, for instance, the diameter of the larger planet is twice as great as that of the smaller, and therefore the surface four times as great, and the volume (or mass, if the planets are of nearly the same density) eight times as great as that of the other, it is evident that as the quantity of heat is proportional to the quantity of matter, or eight times as great in the larger planet when the two are at the same temperature, while the rate of emission, being proportional to the surface, is but four times as great, the supply of heat in the larger will last twice as long as the smaller supply of heat in the smaller planet. Now, as I have said, this reasoning applies equally to the stars ; and if we could only be assured that at any given time two stars of unequal size were in exactly the same stage of stellar life, we should be sure that at any much later stage the smaller star would be much more advanced in stellar life than the larger.

The difficulty arises here, however, that we have no means of proving, but on the contrary strong reason for doubting, whether the stars of our galaxy began their existence of stars at any common time. When we see the various orders of nebulous masses within the galaxy, and note how very different seem to be the states of these nebulæ as to condition, while the very existence of true nebulæ (many of which we may regard as unformed suns or star-clusters) indicates the great diversities of age existing among the occupants of stellar space, we perceive how very unsafe it would be to assume that the stars, simply because they are stars, began their existence as such all at the same or nearly the same time. The contrary is not only far more probable, but to all intents and purposes certain.

All we can safely assume is, that the greater size and mass of a star indicates the much longer continuance of all the stages of its career, past and to come—that it has been much longer in passing through the inchoate stage, and through its first stages as a formed sun; and that it will be very much longer in passing through all those stages which it has still to go through, than our own sun or other suns of the same class. Looking at such a sun as Sirius, for example, we may believe that at the beginning of its present stage of existence as a bluish-white sun, our sun and Sirius may have both been bluish-white, but that our sun, being very much smaller, has passed onwards into the stage when a star shines with yellowish-white lustre, and will perhaps pass onwards to the later stages of which we have yet to speak, while Sirius and Vega are still shining as bluish-white stars. But we cannot assume that any small bluish-white star which gives (as many small stars do) the same sort of spectrum as Sirius, is in reality an enormously large sun, another Sirius in fact, shining with the same sort of light because, beginning its existence at about the same epoch, it has taken a much longer time than our sun to reach the same stage of sun life. It may be that a bluish-white star, with strong hydrogen lines in its spectrum, is no larger than our sun, or is even smaller than he is; but having come into existence as a sun much later, has not reached the same stage of development.

It is important that we should not here fall into an error of the same sort as that which vitiated the earlier reasonings of Sir William Herschel respecting the stellar distances. He regarded the brightness of a star as fairly indicating its distance, assuming all stars to be of the same general order;

later we see a tendency on his part to fall into the opposite extreme, and regard brightness as rather indicating the real size of a star than proximity. Neither inference can in point of fact be relied upon ; some faint stars are large ones very far off ; others are really small stars not farther off, or even nearer, than their fellows.

So it is in the case before us. Some bluish-white suns with spectra indicative of stellar youth are no doubt enormously large orbs, compared with which our sun is little more than as a dwarf compared with a giant ; such suns are young because they are large ; the stages of their lives are all very much shorter than the corresponding stages of the lives of our sun. But others no doubt of these young suns are really young in years as well as in development ; they are younger than our sun, not because they require longer time-intervals for the various stages of their life, but because they began their stellar life later.

Note in passing that these spectra of the bluish-white stars are not all exactly alike. They are distinguished from each other by the greater or less breadth and diffuseness of the lines of hydrogen, and also by various degrees of strength and visibility of the finer lines. It may possibly be that hereafter, in such distinctions as these, we may be able to recognise evidence as to the size of a star—that, for instance, a large star in passing through the first stage of stellar life may present characteristics always different in certain respects from those presented by smaller orbs in passing through the same stage. If so, we shall have a new means of dealing with the architecture of the heavens ; for, knowing something of the real size of a star in this way, we may infer its distance from its apparent size, and thus place it correctly in position in space, instead of knowing only the direction in which it lies, at some distance unknown.

Pass now to the next order of suns, of which Aldebaran, Capella, and our own sun are examples. 'In the spectrum of Aldebaran,' says Dr. Huggins, 'the lines of hydrogen are reduced to about the proportion they possess in the solar spectrum ; the other lines of the spectrum are no longer fine and difficult to see ; we have in full the triple line of magnesium.' I have seen the spectrum of Capella as photographed by Professor Henry Draper of New York, for comparison with the spectrum of the sun, as received after reflexion from the surface of Jupiter. Matters were so arranged that the two spectra were of the same strength. Now when these photographs were placed side by side (the corresponding dark lines being brought into the same direction, so that the eye could run along a dark line of Capella into the corresponding dark line of the sun, I found it almost impossible to recognise the slightest difference between the two spectra. Almost every line in the spectrum of Capella corresponded with a dark line in the spectrum of the sun ; in each case the strength of the lines corresponded very closely. Only after a prolonged and close scrutiny could I satisfy myself that one or two lines of the solar spectrum seemed slightly stronger than the corresponding lines in the spectrum of Capella, and in these cases I found that these very lines are known to be slightly strengthened by absorptive action experienced as they pass through the atmosphere of Jupiter. In this case, apart from a slight disturbing influence due to this absorptive action, a comparison was made between our sun and Capella, precisely as from a world travelling round a sun equidistant from these two orbs. The practical identity of the two spectra is the best proof yet afforded of the oneness of constitution (with infinite variety of distribution) throughout our galaxy.

Again, however, we find ourselves confronted by a difficulty akin to that already experienced in dealing with the question of the relative dimensions of the bluish-white stars. Only that, whereas in their case we could only recognise the extreme probability that many stars of that order differ largely in size from Sirius and Vega, we have in the case of stars of the second order not only probable inference of this sort, but proof positive that two at least among the stars of the second order differ exceedingly in size from our own sun.

For although we do not know the actual distance of either Capella or Aldebaran (I disregard utterly all the measurements of Capella's distance which are given in our books of astronomy, or rather I regard these as proving conclusively that Capella lies utterly beyond the range of measurement [1]), we do know certainly that our sun placed at the distance of either of these stars would shine with very much less light than either of them. We know that, set beside Alpha Centauri, he would shine with about a third part of the light of that star. Now, Capella shines with almost exactly half the light of Alpha Centauri, and Aldebaran with about three-sevenths. Thus our sun set at the distance of Alpha Centauri would shine with about two-thirds the lustre of Capella, and about seven-ninths the lustre of Aldebaran. But each of these stars is at least five times farther away than Alpha Centauri, or otherwise the persistent efforts made to determine the distance of each must long ere this have been rewarded with more success than astronomers have hitherto attained in this direction. Thus each would look at least twenty-five times as bright as it actually does if removed from its present distance to that of Alpha Centauri. Therefore Capella may fairly be assumed to give about forty times (roughly) as much light as our sun at the same distance, and Aldebaran at least thirty times as much. But in the case of two stars whose spectra are very similar to the spectrum of our sun, we may fairly assume that (on the average) each square mile of surface gives out about the same quantity of light as (on the average) each square mile of the surface of the sun. It would follow on this assumption, which is not a very bold one, that the surface of Capella is about forty times as large as the surface of the sun, and the surface of Aldebaran about thirty times as large—say, for convenience of calculation, thirty-six instead of forty in the former case, and twenty-five instead of thirty in the latter. Then it would follow that the diameter of Capella is six times as great, that of Aldebaran five times as great, as the diameter of our sun. Hence the volume of Capella would be (216 times) more than 200 times, and the volume of Aldebaran (125 times) more than 100 times our sun's. Of course the calculation is very rough, and a great deal is assumed. Albeit nearly all the assumptions have been such as rather to diminish than to increase our estimate of the size of these seemingly giant suns of our sun's own order. It is certain Capella and Aldebaran are at least five times farther away than the sun—they may be very much farther away even than that. There is no room for doubt about the photometric measurements by which the relative brightness of the sun, Capella, and Aldebaran, at the same distance, has been determined. It may be, perhaps, doubtful whether the intrinsic brightness of the surface of our sun is so nearly the same as that of the surfaces of Capella and Aldebaran as to leave the estimate we have formed appreciably unaffected by whatever correction may be due to this cause; but be it noticed that we have already made a correction, since we have reduced the estimate of Capella's surface from forty to thirty-six times, and that of Aldebaran's from thirty to twenty-five times, that of the sun's surface.

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[1] It is singular that any faith should be placed by professional astronomers in measurements so manifestly untrustworthy as those which have been given in the case of stars like Capella, Polaris, and Arcturus. When we remember that the star 61 Cygni, which comes next in distance—*probably*—to Alpha Centauri, was first set by the careful measurements of Bessel some three times as far away, and then brought by the equally careful and refined measurements of Peters to only twice the distance of Alpha Centauri—a correction of twenty millions of miles, or more than three years' light journey, we see how utterly unreliable must be estimates like those (due to Peters) which set Arcturus about eight times, Polaris about fourteen times, and Capella about twenty-one times as far away as Alpha Centauri. The error in the determination of the annual displacement of 61 Cygni was fully one-sixth the annual displacement of the nearest star in the heavens—Alpha Centauri—the only star in my opinion whose distance has been fairly, though roughly, measured. Of what use, then, to give us the annual displacements of the three stars named, when even that assigned to Arcturus is only the eighth of that nearest star's—that is, the whole displacement which Peters claimed to have observed in the case of that star—only three-fourths of the *discrepancy* between his result and Bessel's in the case of another star?

Now, if Capella really has a diameter six times greater than the sun's, every stage in the cooling of Capella—that is, every stage of this star's life—would probably last about six times as long as the corresponding stage in the lifetime of our sun. For the volume being on this assumption 216 times as great, it would be in that degree that the quantity of heat in Capella, at any the same stage of its existence, would exceed the quantity of heat in the sun, whether we consider actual or potential heat arising from the contraction due to gravity. The heat would pass away from a surface only 36 times greater, that is, not 216 times as fast (which would make the supply last just as long, but at one-sixth that rate); therefore the supply would last about six times as long. In the case of Aldebaran the supply for each stage of star-cooling would last about five times as long. These numbers are, of course, very far from exactness; but they suffice to show that the lifetime of one star of a given class or order may exceed very much in duration that of another star of the same kind.

We come next to the stars or suns of the third order, whose light, instead of being bluish-white like that of Sirius or Vega, or yellowish-white like that of Capella or of our sun, is of an orange-yellow tint. The best representative of this class of sun is Arcturus, whose spectrum is somewhat like that of our own sun, but presents characteristic peculiarities, which the late Father Secchi regarded as corresponding to what we might expect in a sun like ours at a time when a great number of spots were present on its surface. If we adopt this opinion, we should regard Arcturus as a permanently spotted sun. Dr. Huggins merely remarks of Arcturus that it is a star of another order, which includes the solar type, but the star seems to be removed farther than the sun is in the order of change from the typical form as we meet it in Vega and Sirius. Here the typical lines are no longer present as a strong group. The line which has been regarded as belonging to both calcium and hydrogen is stronger, relatively, than in the solar spectrum. The spectrum of this star is crowded with fine lines, and in the visible part resembles the solar spectrum, but in the ultra-violet part, which hitherto alone photography has recorded, the lines are more intense than in the solar spectrum, and are differently grouped.

The inference from the observed peculiarities of the spectrum of the star Arcturus is that this is a sun further advanced in sun-life than our own.

Now, here again the question as to size is answered in a way suggesting that there is no present correlation between the size of a star and its age or state of development. So far as size is concerned, Arcturus, if it had begun its existence as a sun at the same time as our own sun, should have been much less advanced than he is. For Arcturus is half as bright again as Capella, yet lies at least as far away as that distance which we have assigned as the least possible distance for Capella. Therefore all that we have said about Capella and Aldebaran applies with increased force to Arcturus. His surface is probably at least sixty or seventy times as large as Capella's, even if we assume that the intrinsic brightness of the surface of this older star is equal to that of our sun's surface; but it is probably less, in which case to account for the great *amount* of light emitted by Arcturus we must assume the surface to be greater in proportion as its intrinsic brilliancy is less. Even with a surface only sixty-four times as great as the sun's, Arcturus would have a diameter exceeding his eight times, and a volume exceeding his nearly five hundred times. Arcturus would therefore be a sun marvellously surpassing our own in volume, and presumably in mass also. We may infer, reasonably enough, that the family of worlds over which this mighty orb bears sway surpasses in like degree in dignity and importance that ruled over by our own sun.

In passing, let it be noticed that all these considerations as to the great size of many, if not most, of the stars of the first orde (the bluish-white suns), of some at any rate of the stars of the second order (the yellowish-white suns), and of one at least of the stars of the third order (the orange-yellow suns) are enormously, one may say overwhelmingly, strengthened, if we accept Dr. Siemens' view of the exhaustion of each sun's rays as they do their work in space. For in that case all the stars must emit very much more light than we have been assuming that they do. In fact, if that theory were true, the mere visibility of a star at the distance of Sirius would imply that the sun so seen across depths of space exceeding at least a million times the entire span of the earth's orbit, was an orb compared with which our sun is less than the tiniest meteor compared with the mighty mass of our earth. For our own sun, if he does anything like the work assigned him by Dr. Siemens, must exhaust all his light-giving as well as heat-giving energies long before he can extend the news of his existence as a sun even to the distance of the nearest star. Yet *there* in the star-depths are ten thousand suns which do much more than merely make themselves visible athwart such distances, some of them even giving hundreds of times as much light as our sun would give if—without any such exhaustion of his rays in space—he shone from beyond such distances as separate those orbs from us.

But apart from all such questions as these, there is to me something most impressive in the thought of what, as thus interpretated by spectrum analysis, the heavens reveal to us. Of old it was known that one star differs from another in glory—meaning perhaps in brightness only. In colour, too, it had been seen that the stars are unlike. But who would have ventured to surmise that in real size the suns that people space are so unlike? Who could have supposed that any instruments men could devise would enable us to judge which are the younger, which the older stars? Yet even the most cautious among our astronomical physicists, Dr. Huggins, ablest of our spectroscopists, accepts this as the only reasonable solution of the observed differences in star spectra. 'We cannot resist,' he says, 'the feeling that in Arcturus' (and the other stars of that class) 'we have to do with a star which has departed farther from the condition in which Vega now is than our sun has yet done. The question presents itself, Have we before us stars of permanently different orders, or have we to do with some of the life-changes through which all stars pass? Does the sun's position, somewhere before Arcturus in the order of change, indicate also his relative age? On these points we know nothing certainly.' 'If I may give some play to the scientific use of the imagination,' he added, addressing his audience at the Royal Institution, 'I would ask you to imagine an inhabitant from some remote part of the universe, seeing for the first time an old man with white hair and wrinkled brow, to ask, "Was he born thus?" The answer would be, "No; in this child, this youth, this man of mature age, you see some of the life-changes through which the old man has passed." So, giving play to the scientific imagination, there may have been a time when a photograph of the solar spectrum would have presented the typical lines only which are still in Vega. At a subsequent period these would have been narrower and more defined, and other lines would have made their appearance. And if we allow this scientific imagination to project these Royal Institution Friday evenings into the far future, the lecturer, clad it may be in the skin of a white bear, may have to describe how the spectrum of the then feeble sun has already passed into the class of spectra distinguishing those stars which shine with red light.'

It is evident that our great astronomical physicist recognises no perpetual energy in suns, even in the mightiest. He sees them passing downwards along the scale of stellar being, gradually parting with more and more of their stored-up energies, not recruiting themselves with their own energies stored up after doing their full work. And in this, with all respect to an eminent practical physicist, he shows himself more philosophical as well as more practical. He recognises that the same law which affects the small and the short-lived, the large and the long-lived must also submit to. Practically eternal though to our conceptions the duration of each stage of a sun's life may be, each such stage is nevertheless finite, even though a sun exceed our own a million times in volume or in mass. The heavens present to us a scene of tremendous —nay, of inconceivable energy. Suns upon suns, to millions, to tens of millions, to hundreds, even to thousands of millions, occupy space around us. In every stage of stellar life they are at work, illumining, heating, and guiding the systems which circle around them. Beyond the limits of the most powerful telescope lie thousands of millions more, repeating the same story of seemingly infinite energy, seemingly endless duration. Yet each one of those orbs, and therefore the sun of all, or the universe as we know it, tends to an end—an end which may be, however, but the beginning of new forms of existence, while the gaseous nebulæ, now mere masses of vapour, may then have entered on sun-life, to carry on the same story, to teach the same lesson, that though each order of created things tends onwards to an end, yet to such orders we can trace no visible limit—'End is there none to the universe of God; lo, also, there is no beginning.'

THE END.

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